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EDITORIAL

TWO YEARS have now elapsed since the founding of the Econometric Society. Although the Society has purposely given little publicity to its affairs during these years of organization, inquiries and suggestions have come from many quarters manifesting a readiness for, and a keen expectation of, something along the lines now followed by the Econometric Society. A source of potential energy, much larger than originally anticipated by the founders of the Society, seems to exist, only waiting to be released and directed into econometric work.

This is the reason why the Society has decided to establish its own journal. ECONOMETRICA will be its name. It will appear quarterly; this is the first issue.

A word of explanation regarding the term econometrics may be in order. Its definition is implied in the statement of the scope of the Society, in Section I of the Constitution, which reads: "The Econometric Society is an international society for the advancement of economic theory in its relation to statistics and mathematics. The Society shall operate as a completely disinterested, scientific organization without political, social, financial, or nationalistic bias. Its main object shall be to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences. Any activity which promises ultimately to further such unification of theoretical and factual studies in economics shall be within the sphere of interest of the Society."

This emphasis on the quantitative aspect of economic problems has a profound significance. Economic life is a complex network of relationships operating in all directions. Therefore, so long as we confine ourselves to statements in general terms about one economic factor having an "effect" on some other factor, almost any sort of relationship may be selected, postulated as a law, and "explained" by a plausible argument.

Thus, there exist a real danger of advancing statements and conclusions which—although true as tendencies in a very restricted sense—are nevertheless thoroughly inadequate, or even misleading if offered as an explanation of the situation. To use an extreme illustration, they may be just as deceptive as to say that when a man tries to row a boat forward, the boat will be driven backward because of the pressure exerted by his feet. The rowboat situation is not, of course, explained by finding out *that* there exists a pressure in one direction or another, but

only by comparing the relative *magnitudes* of a number of pressures and counter-pressures. It is this comparison of magnitudes that gives a real significance to the analysis. Many, if not most, of the situations we have to face in economics are of just this sort. The full usefulness of a large and important group of our economic analyses will come, therefore, only as we succeed in formulating the discussions in quantitative terms.

But there are several aspects of the quantitative approach to economics, and no single one of these aspects, taken by itself, should be confounded with econometrics. Thus, econometrics is by no means the same as economic statistics. Nor is it identical with what we call general economic theory, although a considerable portion of this theory has a definitely quantitative character. Nor should econometrics be taken as synonomous with the application of mathematics to economics. Experience has shown that each of these three view-points, that of statistics, economic theory, and mathematics, is a necessary, but not by itself a sufficient, condition for a real understanding of the quantitative relations in modern economic life. It is the *unification* of all three that is powerful. And it is this unification that constitutes econometrics.

This unification is more necessary today than at any previous stage in economics. Statistical information is currently accumulating at an unprecedented rate. But no amount of statistical information, however complete and exact, can by itself explain economic phenomena. If we are not to get lost in the overwhelming, bewildering mass of statistical data that are now becoming available, we need the guidance and help of a powerful theoretical framework. Without this no significant interpretation and coordination of our observations will be possible.

The theoretical structure that shall help us out in this situation must, however, be more precise, more realistic, and, in many respects, more complex, than any heretofore available. Theory, in formulating its abstract quantitative notions, must be inspired to a larger extent by the technique of observation. And fresh statistical and other factual studies must be the healthy element of disturbance that constantly threatens and disquiets the theorist and prevents him from coming to rest on some inherited, obsolete set of assumptions.

This mutual penetration of quantitative economic theory and statistical observation is the essence of econometrics. And therein lies the need for mathematics, both in the formulation of the principles of economic theory, and in the technique of handling the statistical data. Mathematics is certainly not a magic procedure which in itself can solve the riddles of modern economic life, as is believed by some enthusiasts. But, when combined with a thorough understanding of the economic

significance of the phenomena, it is an extremely helpful *tool*. Indeed, this tool is indispensable in a great many cases. Many of the essential things in the new setting of the problems are so complex that it is impossible to discuss them safely and consistently without the use of mathematics.

To stimulate work in the directions here indicated is the purpose of the Econometric Society and its journal ECONOMETRICA. ECONOMETRICA will report on research, teaching, scientific meetings, and other activities of econometric interest in the various countries. It will publish original papers that are significant, directly or indirectly, as contributions towards the development of econometrics. The history of our subject will also receive attention. No effort will be made, however, to preempt all the important econometric contributions for the columns of this journal. On the contrary, ECONOMETRICA will encourage, and probably from time to time report on, econometric papers in all the leading economic journals of the world. The general policy will be to make ECONOMETRICA a *clearing house* for econometric work.

As time goes on, some form of cooperation may perhaps develop. ECONOMETRICA may, for instance, be ready to take over for publication papers that are so mathematical as to be unsuitable for publication in certain other economic journals. Indeed, it will be an editorial principle of ECONOMETRICA that no paper shall be rejected solely on the ground of being too mathematical. This applies no matter how highly involved the mathematical apparatus may be.

This is not, of course, to say that a paper will be rendered acceptable to ECONOMETRICA merely by its employment of mathematical symbols. The policy of ECONOMETRICA will be as heartily to denounce futile playing with mathematical symbols in economics as to encourage their constructive use. And a considerable portion of the material appearing in ECONOMETRICA will probably be entirely non-mathematical.

In statistical and other numerical work presented in ECONOMETRICA the original raw data will, as a rule, be published, unless their volume is excessive. This is important in order to stimulate criticism, control, and further studies. The aim will be to present this kind of paper in a condensed form. Brief, precise descriptions of (1) the theoretical setting, (2) the data, (3) the method, and (4) the results, are the essentials.

At one time we considered organizing in ECONOMETRICA an extensive work on abstracts of econometric articles and books. However, it was found that this would to some extent overlap the work done by other economic journals, and that ECONOMETRICA would do a greater service in organizing *surveys* of the significant developments within the main fields that are of interest to the econometrician. Each year there will appear in ECONOMETRICA four such surveys, one for each of the

following fields: (1) general economic theory (including pure economics), which will appear as a regular feature in the January issue, (2) business cycle theory, in the April issue, (3) statistical technique, in the July issue, (4) statistical information, in the October issue.

All these surveys will be international in scope, but there will be no attempt to make them all-inclusive. On the contrary, they will be selective. Their aim will be to present to the reader an appraisal of the really significant developments in the fields in question, thus being a useful guide for those who are interested in these fields, without having, perhaps, the time to follow the literature closely. There will be one responsible author for each survey, who will seek, when necessary, the cooperation of specialists. The names of the authors of the first four surveys will be found in the Section on Announcements at the end of this issue.

Complete freedom of thought will rule in the columns of ECONOMETRICA, and candid discussion on the surveys or on other material appearing in its pages will always be welcome. To judge from the stimulating discussions that have taken place both at the European and the American meetings of the Society, there is no danger of inbreeding of thought amongst econometricians. It is true that those attending the meetings have shown a veritable enthusiasm for the common cause, econometrics. But, together with this community in general interest and attitude, there has been manifest a variety of ideas and a frankness in mutual criticism that guarantee the broadness and freshness of the future work. This vigorous spirit will, no doubt, be reflected in the columns of ECONOMETRICA.

ECONOMETRICA is presented to the public in the hope that it will be able to do its full share in the constructive work mapped out by the Econometric Society.

EDITOR

THE COMMON SENSE OF ECONOMETRICS

By Joseph Schumpeter

The aims of this Journal, and of the Society of which it is to be the organ, have been stated above by the Editor with that brevity and precision which are characteristic of every statement of a sound case. What I have to add, by way of comment and amplification, will, I hope, confirm the impression that there is nothing startling or paradoxical about our venture, but that it grows naturally out of the present situation of our science. We do not wish to revive controversies about general questions of 'method,' but simply to present and discuss the results of our work. We do not impose any credo—scientific or otherwise—, and we *have* no common credo beyond holding: first, that economics is a science, and secondly, that this science has one very important quantitative aspect. We are no sect. Nor are we a 'school.' For all possible differences of opinion on individual problems, which can at all exist among economists, do, and I hope always will, exist among us.

Like everything else, economic life may be looked at from a great, strictly speaking an infinite, number of standpoints. Only some of these belong to the realm of science, still fewer admit of, or require, the use of quantitative methods. Many non-quantitative aspects are, and always have been, more interesting to most minds. Fruitful work can be done on entirely non-quantitative lines. Much of what we want to know about economic phenomena can be discovered and stated without any technical, let alone mathematical, refinements upon ordinary modes of thought, and without elaborate treatment of statistical figures. Nothing is farther from our minds than any acrimonious belief in the exclusive excellence of mathematical methods, or any wish to belittle the work of historians, ethnologists, sociologists, and so on. We do not want to fight anyone, or, beyond dilettantism, anything. We want to *serve* as best as we can.

ECONOMICS, THE QUANTITATIVE SCIENCE

There is, however, one sense in which economics is the most quantitative, not only of 'social' or 'moral' sciences, but of *all* sciences, physics not excluded. For mass, velocity, current, and the like *can* undoubtedly be measured, but in order to do so we must always invent a distinct process of measurement. This must be done before we can deal with these phenomena *numerically*. Some of the most fundamental economic facts, on the contrary, already present themselves to our observation as quantities made numerical by life itself. They carry meaning only

by virtue of their numerical character. There would be movement even if we were unable to turn it into measurable quantity, but there cannot be prices independent of the numerical expression of every one of them, and of definite numerical relations among all of them.

Econometrics is nothing but the explicit recognition of this rather obvious fact, and the attempt to face the consequences of it. We might even go so far as to say that by virtue of it every economist is an econometrician whether he wants to be or not, provided he deals with this sector of our science and not, for example, with the history of organization of enterprise, the cultural aspects of economic life, economic motive, the philosophy of private property, and so on. It is easy to understand why explicit recognition of this fact should have been so difficult, and why it has taken so long to come about. Philosophers, who have at all times delighted in classifying sciences, have always felt uneasy about the precise place to be allocated to economics as a whole. As it was, they practically followed the empirical dividing line between 'natural' and 'moral' sciences, and classed economics with the latter. And there, of course, the quantitative aspect or sector of our science found but uncongenial ground.

Another reason was that economic problems have most of the time been approached in a practical spirit, either indifferent or hostile to the claims of scientific habits of thought. No science thrives, however, in the atmosphere of direct practical aim, and even practical results are but the by-products of disinterested work at the problem for the problem's sake. We should still be without most of the conveniences of modern life if physicists had been as eager for immediate 'application' as most economists are and always have been. This accounts for the neglect of econometrics, as well as for the unsatisfactory state of our science in general. Nobody who craves for quick and short answers to burning questions of the day will care to entangle himself in difficulties which only patient labor can clear in the course of many years.

Nevertheless, the quantitative character of the subject was bound to assert itself. It is one of the most striking facts about the history of our science, that most—and if we exclude historians, *all*—of those men whom we are justified in calling great economists invariably display a remarkably mathematical turn of mind, even when they are entirely ignorant of anything beyond the quantitative technique at the command of a schoolboy; Quesnay, Ricardo, Böhm-Bawerk, are instances in point.

Nor is this all. If econometricians have any wish to imitate other people and to glory in heroic ancestry, they may with justice claim the great name of Sir William Petty as their own. The second part of the seventeenth century is full of vigorous ventures into the field of

econometrics—one has but to point to Gregory King's statistical demand curve. It is a question of some interest how it was possible that such hopeful beginnings could have failed to inspire further work, and how their results could have been left to linger in the dusk, although they were by no means forgotten, the reference to 'King's rule' being part of the stock in trade of almost every stale textbook written ever since.

In the sphere of monetary phenomena and its neighborhood, quantitative and even numerical analysis became established practice as far back as the sixteenth century, mainly in Italy, and this tradition was never lost again; passages in the Italian writers of the eighteenth century, such as Beccaria, Carli, Verri, and others, sounding very familiar to the modern ear. What we have before us there is nothing less than a conscious attempt to weld into one indivisible argument theorems and statistical facts.

And, provided we leave out the word 'conscious,' we find substantially the same tendency in whatever piece of the work of our predecessors we choose to look at. To give but one example; we are accustomed to scoff at the literature of the time-honored controversy on Value. But what else is at the bottom of it, overlaid it is true by heavy masses of speculative verbiage, if not the truly scientific search for an economic unit of measurement, or of several such units adapted to different classes of phenomena? There was no more un-empiric speculation about it than there is about every science in its infancy. Nor is there less connection with such statistical materials as each epoch could command than we are entitled to expect—as everyone will admit who has taken the trouble of perusing Ricardo's reply to Bosanquet.

LATER DEVELOPMENTS

Essentially quantitative analysis, but crippled by the lack both of appropriate technique and of adequate statistical material—this is the diagnosis we arrive at when we study the work of economists up to that time, when Mill's principles were fairly representative of what our science had to give. This, too, is the element of truth which emerges from the hostile phraseology which we are in the habit of using about 'classical doctrine.' Obviously, therefore, what our society stands for is anything but an innovation. It is no more than a conscious endeavor to remove obstructions to the flow of a stream which has been running ever since men began to think and write about economic life.

In order to see in their full significance the conditions which have made it desirable, and indeed necessary, to form, under the banner of Econometrics, a *coalition* of the different types of economists who are to join hands in our society, we must, however, now glance at later

developments. The phase that could, until about ten years ago, be called the 'modern' phase of economics, admits of description in terms of three facts and their consequences: first, the rapid growth of our wealth of statistical and other material; second, the progress of statistical technique at our command—which so far as it grew up largely outside our own field and without reference to our needs, was a stroke of good fortune, very much as a lift in another man's car is to the wanderer on a dusty road; and third, the emergence of a theoretical engine very much superior to the old one. True, on none of these heads are we, or can we ever be, satisfied; on every one of them, it seems to me, the real thing is still to come, and present performance calls for apology rather than congratulation. Yet it would not only be ungenerous, but positively false, to deny the importance of what has been achieved, or the possibilities which begin to loom in the future.

In all this the econometric line stands out clearly. It was definitively established that economic theory involves quantity, and therefore requires the only language or method available to deal with quantitative argument as soon as it outstrips its most primitive stage. To W. St. Jevons belongs the honor of having spoken one of those simple messages, which at times seem to focus both past and future history and to become mile-stones visible ever after. It was he who said in the introduction to his *Theory of Political Economy* (1st ed. 1871); "It is clear that Economics, if it is to be a science at all, must be a mathematical one." But still higher tribute is due to A. Cournot who, without encouragement or lead, in what was then a most uncongenial environment, in 1838 fully anticipated the econometric program by his *Recherches*, one of the most striking achievements of true genius, to which we pay respect to this day by nearly always starting out from them. Of course, it would be superfluous to emphasize the paramount importance of that great teacher of ours whose exposition of exact theory sprang from his head as did Minerva from the head of Jove. What I want to stress is that he constructed his analytic apparatus with a clear perception of the ultimate econometric goal, every part of it being thought out so as to fit it to grip statistical fact when the time should come. In this he went much farther than Jevons. This reads like a paradox because Jevons actually worked on figures, as in the matter of index numbers. But within the precincts of pure theory itself, he seems much less concerned with that goal than Cournot, and it is much more difficult for the numerical horse to jump Jevons' fences than it is to trot on Cournot's road.

In our pantheon, J. H. von Thünen's place is side by side with Cournot's. It is not only—indeed not even primarily—the idea of marginal productivity which it is important to mention here, but Thünen's pe-

culiar relation to a set of facts, which is as vital to econometrics as statistics in the narrow sense of the word. Thünen pointed out that cost accounting, bookkeeping, and neighboring headings, cover a mass of material which economists have entirely neglected. This neglect has indeed been so complete that the specialists of 'Business Administration' now actually have begun to build their own theoretical houses which wall them off from 'general theory' as completely as it, in turn, had excluded them, in spite of the fact that both groups of workers to a great extent—a signal instance is the matter of cost curves—till the same ground. It is clear that economists cannot indefinitely do without that vast reservoir of fact, nor cost accountants, bookkeepers, and so on, do without the economists' contribution. And, looking back, we see now that as early as 1826, Thünen's book could have taught us *how theory grows out of the observation of business practice.*

I for one shall always look up to Léon Walras as the greatest of all economists. In his theory of equilibrium he gave a powerful basis to all our work. It is true that while he made the decisive step in the *quantitative*, he failed to move in the *numerical* line, the *junction* of which two is characteristic of econometrics. But we have been taught of late to look more hopefully even on the 'numerical' possibilities of that most general and most abstract part of our science which is equilibrium theory in Walras' sense. And this fact similarly indicates the econometric claims of the work of Auspitz and Lieben, of Knut Wicksell, of Francis Y. Edgeworth, and of Walras' great successor in Lausanne, Vilfredo Pareto.

In a somewhat different sense, we may finally claim as our own that greatest of all teachers of economics, Alfred Marshall. With some of us, it has become a custom to speak of him as the exponent of 'neoclassical' doctrine. This is not the place to show how it came to pass—not without some fault of Marshall himself—that so utterly unjust, and in fact meaningless, a label was affixed to his name. But I wish to emphasize first, that nobody can peruse his address on "The Old Generation of Economists and the New" without discovering, though not perhaps without some surprise, how clearly our program stood before his mind. Nor is it possible for anyone who knows how to read his *Principles* in the light of his *Industry and Trade* to define what he really strove to accomplish in any but econometric terms. Most important of all, he always worked with an eye to statistical application, and he was at his best *as a theorist* when constructing those handy tools, like elasticity, quasi-rent, external and internal economies, and so on, which are so many bridges between the island of pure theory and the terra firma of business practice and business statistics.

I do not wish to speak of any living economists. But the readers

would probably not forgive me if I failed to make two exceptions, and to mention the pioneer work of Irving Fisher and Henry L. Moore.

THE PRESENT STATE

All these achievements were, to say the least, enough as a good start and to build up from. And, indeed, work full of promise has been done in our line during the last two decades, work which makes us feel, when we now look at the Walrasian system, very much as we feel when beholding the model of a motor car constructed forty years ago. But still, most of us undoubtedly do agree in finding the present state of our science disappointing, not only in comparison with the achievements of other sciences, but also in comparison with what our science could fairly be expected to perform. There are many reasons for this, but some of them only, having special bearing on the mission of this Society, call for attention here.

Reasoning on economic facts means, and always meant, within a very important sector, quantitative reasoning. And there is no *logical* breach between quantitative reasoning of an elementary character, and quantitative reasoning of the kind involving the use of 'higher mathematics.' But nothing makes a greater *practical* breach in the evolution of a science than the introduction of a habit of thought which has so far been foreign to the recognized equipment of the specialist, and which at the same time is inaccessible except by strenuous effort. When the necessity of proceeding to the use of more refined mathematical methods, both in economic theory and in statistics, became apparent to some, the majority even of those economists who did work the quantitative sector refused to follow. At first they laughed. They do so no longer. Integrals cease by and by to be as hieroglyphs to them. Many of them try to understand and have made their peace with us, while reserving their right to criticize our results and to object to mathematical excesses. But this is not the full cooperation we need. Even in this improved situation, economics lacks that broad expanse of professional common ground which, in the case of physics, transmits acquired results to the general public. Beginners are bewildered by this unsettled situation. Energy is being wasted and the real business of the science hampered. Recent progress, and still more than *actual* progress wide *possibilities* of it, has drawn to our field a most promising host of newcomers. But the old situation being fundamentally changed, we had no uniform training to offer them. Hence the lack of coordination of the work. The new men came to face our problems from very different angles and with very different acquirements, full of impatience to clear the ground and to build entirely anew. The man whom nature had moulded to delight in unadulterated fact, whether he worked in

a statistical bureau or did field work on his own, often knew little and cared less for that engine of analysis, which we call 'economic theory,' or for refined statistical technique. On the other hand, the master of this technique, feeling its power and seeing the material to grip with it, tried to rush at his own kind of regularities or generalizations. And the theorist, conscious of his own task, refused more often than was wise to accept the work of the other two types as anything but (possible) verifications of his theorems. But, although uncoordinated, the growth has been tropical. It might be expected to settle down and bear fruit in time, but there is chaos for the present, in which only a very experienced eye can see an underlying tendency working its way slowly though powerfully towards a goal common to all.

THE PROGRAM

The common sense of the program of our Society centers in the question: Can we not do better than this? Surely it would not be a reasonable policy to sit down and wait till, in the end, things find their level by themselves, and meanwhile to allow econometricians of all countries to fight single-handed their uphill battle. What we want to create is, first, a forum for econometric endeavor of all kinds wide enough to give ample scope to all possible views about our problems, yet not wide enough to be hampered by the weight of an audience which keeps discussion in the ante-rooms of the real points at issue, and forces every speaker or writer to go every time over the same preliminaries.

On this forum, which we think of as international, we want secondly to create a spirit and a habit of cooperation among men of different types of mind by means of discussions of concrete problems of a quantitative and, as far as may be, numerical character. The individual problems themselves are, as it were, to teach us how they want to be handled. We want to learn how to help each other, and to understand *why*, and precisely *where*, we ourselves, theorists, statisticians, collectors of facts, or our neighbors, do somehow not quite get to where we want to be. No general discussion on principles of scientific method can teach us that. We have had enough of it. We know it leads nowhere, and only leaves the parties to the contest where they were before, still more exasperated perhaps by those gentle rudenesses it is customary to administer to each other on such occasions. No general arguments of this kind ever carry conviction to the man who means real work. But, confronted with clear-cut questions, most of us will, we hope, be found to be ready to accept the only competent judgment on, and the only relevant criterion of, scientific method, that is the judgment or criterion of the result. There is high remedial virtue

in quantitative argument and exact proof. That part of our differences —no matter whether great or small—which is due to mutual misunderstanding, will vanish automatically as soon as we show each other, in detail and in practice, how our tools work and where they need to be improved. And metaphysical acerbity and sweeping verdicts will vanish with it. Theoretic and 'factual' research will of themselves find their right proportions, and we may not unreasonably expect to agree in the end on the right kind of theory and the right kind of fact and the methods of treating them, not postulating anything about them by program, but evolving them, let us hope, by positive achievement.

We should not indulge in high hopes of producing rapidly results of immediate use to economic policy or business practice. Our aims are first and last scientific. We do not stress the numerical aspect just because we think that it leads right up to the core of the burning questions of the day, but rather because we expect, from constant endeavor to cope with the difficulties of numerical work, a wholesome discipline, the suggestion of new points of view, and helps in building up the economic theory of the future. But we believe, of course, that indirectly the quantitative approach will be of great practical consequence. The only way to a position in which our science might give positive advice on a large scale to politicians and business men, leads through quantitative work. For as long as we are unable to put our arguments into figures, the voice of our science, although occasionally it may help to dispel gross errors, will never be heard by practical men. They are, by instinct, econometricians all of them, in their distrust of anything not amenable to exact proof.

Harvard University

Antoine Augustin Cournot
1801–1877

COURNOT ET L'ÉCOLE MATHÉMATIQUE

Par René Roy

L'œuvre économique de Cournot comprend seulement deux ouvrages: (1) *Recherches sur les principes mathématiques de la théorie des richesses*, publié en 1838, et (2) *Principes de la théorie des richesses* publié en 1863. Le second ouvrage ne contient d'ailleurs aucun développement mathématique et reprend, en les complétant, les idées exposées dans le premier; celui-ci contient donc en substance tous les éléments des problèmes traités par Cournot.

Pour apprécier l'importance de la contribution de notre auteur aux progrès de la science économique, il suffit de rappeler l'opinion de Léon Walras, le fondateur de l'École de Lausanne, qui, lisant en 1873, devant l'Académie des Sciences morales et politiques de Paris, son mémoire sur l'Échange, proclamait que Cournot était le premier auteur qui eût tenté franchement et sérieusement d'appliquer les mathématiques á l'Économie politique; Walras ne dissimulait d'ailleurs pas que la lecture des *Recherches* avait fait une forte impression sur son esprit, en l'orientant vers les études qui devaient le conduire à formuler les principes de l'équilibre économique.

De même, Stanley Jevons, qui prend connaissance, en 1872, du premier ouvrage de Cournot, reconnaît en lui un précurseur, et signale ses travaux à l'attention du monde savant. De son côté, Marshall insiste sur l'influence qu'a eue l'auteur français sur ses propres travaux et il considère comme regrettable que les études de Cournot, complétées plus tard par celles de Dupuit, aient été trop longtemps tenues à l'écart par les travaux de l'École anglaise sur le coût de production. Enfin, le professeur Edgeworth considère que les énoncés de Cournot constituent la meilleure présentation mathématique de certaines des plus hautes généralisations de l'Économie politique.

La valeur de ses travaux n'est donc contestée par personne, bien que Cournot n'ait pas été le premier, qui se soit essayé á présenter les phénomènes économiques au moyen de l'appareil mathématique; parmi ses précurseurs, deux seulement paraissent devoir retenir l'attention: Isnard, Ingénieur des Ponts-et-Chaussées, qui, en 1781, publia un *Traité des Richesses*, et Von Thünen, dont le premier ouvrage, publié en 1826, peut être également considéré comme susceptible de marquer une date dans l'histoire de l'Économie mathématique.

Mais l'œuvre de Cournot ne se limite pas au seul domaine économique, et pour apprécier sainement l'importance de sa contribution en cette matière, il est indispensable de procéder à un inventaire, même sommaire, des ouvrages qu'il publia aux diverses époques de sa vie;

esprit sagace et penseur profond, Cournot développa sa personalité à la fois dans les domaines des mathématiques pures, de la philosophie, de l'histoire, et de la sociologie.

LA PENSÉE DE COURNOT

Cournot, né en 1801, est mort en 1877; il fit de fortes études mathématiques qui le conduisirent á l'École Normale, puis un peu plus tard, à l'enseignement des mathématiques. En dehors de sa thèse de doctorat, il publia même deux ouvrages de mathématiques pures: en 1841 le *Traité élémentaire de la théorie des fonctions et du calcul infinitésimal* et en 1843 l'*Exposition de la théorie des chances et des probabilités.*

Cependant, il fut de bonne heure attiré par les questions historiques et sociales; il étudia même le droit et se signala en philosophie par la publication de l'*Essai sur le fondement de nos connaissances et sur le caractère de la critique philosophique*, paru en 1851, et surtout par le *Traité de l'enchaînement des idées fondamentales dans les sciences et dans l'histoire* ouvrage publié en 1861.

Les divers courants de sa pensée ont influé dès 1838 sur la rédaction de son premier essai dans le domaine économique, et nous pouvons ainsi nous rendre compte de la nature des principes généraux auxquels il a recouru; nous verrons que ces principes sont liés de très près à sa formation scientifique.

Cournot distingue en matière de sciences, d'une part ce qu'il appelle les sciences de la quantité, comme la physique, d'autre part, les sciences des combinaisons, auxquelles se rattachent les sciences sociales; les premières requièrent peu de vérifications expérimentales, tandis que les secondes exigent un contrôle permanent des faits, pour apprécier leur conformité aux théories. Pour notre auteur, les lois scientifiques sont essentiellement contingentes, et dans l'ordre économique en particulier, les rapports qui s'établissent au sein des sociétés ont pour effet d'éliminer peu à peu l'influence des phénomènes accidentels, pour ne laisser apparaître, dans un stade d'évolution suffisamment avancé, que des relations strictement conformes à l'énoncé des lois qui gouvernent ces phénomènes sociaux; Cournot estime ainsi, qu'un jour il sera possible de préciser exactement l'influence d'une variation subie par le prix d'un article sur son débit.

Cette perspective d'un progrès indéfini pour les sciences des combinaisons tient d'ailleurs à ce que les observations portent sur une très grande masse d'individus à laquelle on peut appliquer la loi des grands nombres. Notre auteur, qui était en relations d'amitié avec Poisson, n'ignorait pas tout le parti que Laplace avait tiré de la notion de probabilité, et il ne doutait pas que semblables principes pussent être envisagés comme bases des études économiques. Seulement, pour lui,

la notion de probabilité résidait dans la nature même des choses, en dehors de toute conception philosophique, tandis que pour Laplace, elle ne traduisait que l'imperfection de nos connaissances. Quoiqu'il en soit, Cournot a pu fort heureusement recourir à la loi des grands nombres pour utiliser dans le domaine social des fonctions mathématiques indéterminées, mais continues et susceptibles, par conséquent, de donner aisément prise à l'analyse; nous verrons, en étudiant la loi du débit, tout le parti qu'il a tiré de cette méthode.

Selon Cournot, les lois naturelles doivent avoir une expression simple et leur probabilité est en raison directe de leur simplicité. Ce sont là encore des vues en harmonie avec les tentatives contemporaines de synthèse dans l'ordre scientifique. L'importance de la loi des grands nombres dans le domaine économique conduit d'ailleurs Cournot à préconiser l'usage des statistiques, afin d'éliminer l'influence des événements fortuits; parmi les causes de variation, subsistent seulement celles qui présentent le caractère de causes permanentes et stables.

Comme autres principes directeurs, Cournot recourt fréquemment aux lois issues du monde physique, telles que le principe de moindre action, dont l'expression dans le monde économique n'est autre que le principe hédonistique, en vertu duquel les individus s'efforcent d'obtenir le maximum de satisfaction moyennant le minimum d'effort. Il applique de même très heureusement aux problèmes économiques le concept d'équilibre stable, avec toutes ses conséquences, ainsi que celui du mouvement soustrait aux résistances passives, d'où il déduit son marché théorique. Cependant, il est loin d'assimiler les collectivités humaines aux ensembles immatériels considérés par le physicien, et si la notion de force constitue chez lui un ressort très puissant, il tient compte dans une large mesure de l'élément spécifique des sociétés humaines, en faisant intervenir la "force vitale" irréductible aux éléments purement mécaniques; à cet égard, nos idées n'ont guère évolué depuis lors et le vitalisme contemporain diffère peu des théories invoquées par Cournot à propos de l'évolution des sociétés humaines.

En résumé, la pensée directrice de Cournot est essentiellement constituée par la loi des grands nombres et la notion de probabilité, dont personne n'ignore la fortune dans la physique contemporaine et même dans les sciences biologiques.

Quant à l'emploi des mathématiques en Économie politique, c'est là un procédé que Cournot préconise en raison même de sa foi dans la puissance de cette analyse et du caractère évolutif des sciences sociales. Les arguments invoqués à l'appui de semblables méthodes peuvent être ainsi résumés:

a) L'usage des mathématiques conduit seul aux définitions claires et précises ainsi qu'aux exposés simples;

b) l'usage des mathématiques permet également d'utiliser des fonctions, et par conséquent, de résoudre les problèmes de maxima et de minima qui se posent fréquemment en Économie politique.

A ce propos, il est intéressant de noter l'emploi de fonctions non définies par leur expression analytique et simplement assujetties à réaliser certaines conditions comme la continuité, l'existence et le signe de la dérivée.

Il semble que Cournot ait ainsi précédé Henri Poincaré, qui fait constamment usage de telles fonctions dans sa théorie des probabilités; on sait tout le parti qu'il a tiré de cette méthode.

c) Les relations fonctionnelles auxquelles recourt le mathématicien, établissent entre les divers paramètres économiques, des relations de mutuelle dépendance, qui font totalement abstraction du lien de causalité.

Nous touchons ici à l'un des points les plus délicats de la science économique, qui, comme toutes les sciences d'observation, ne peut donner prise à l'expérimentation, et se prête exclusivement à l'étude de phénomènes variant simultanément dans le temps. La logique formelle, qui recourt au syllogisme, ne peut au contraire éliminer ces liens de causalité et ne parvient pas à embrasser tout un ensemble de faits appelés à se modifier constamment en réagissant les uns sur les autres. Cournot, de même qu'Auguste Comte, ne perd jamais de vue le caractère d'unité présenté par les systèmes étudiés, aussi bien dans l'ordre social, que dans l'ordre physique.

Ces multiples considérations, avec lesquelles nous sommes aujourd'hui familiarisés, et qui nous apparaissent même comme de vulgaires lieux communs, étaient en revanche fort nouvelles à l'époque où elles ont été développées par leur auteur, et cette circonstance a été probablement cause de la froideur qui les accueillit.

Nous devons aussi noter que malgré sa foi dans la fécondité de l'appareil mathématique, Cournot pose en principe qu'il est toujours indispensable de confronter la réalité, avec les résultats de toute analyse scientifique, soit pour corriger les prémisses qui ont servi de base, soit pour limiter le champ d'application des conclusions; il est ainsi d'accord avec Henri Poincaré, qui proclamera plus tard, que l'expérience seule est rénovatrice; mais il faut reconnaître que bien souvent, Cournot s'est écarté de ce principe, et que certaines de ses conclusions n'ont pu résister par la suite à l'épreuve des faits.

Ces principes généraux étant posés, il est possible d'étudier les grandes lignes des problèmes économiques traités par Cournot, et tout spécialement ceux qui n'ont pu être entamés par une critique presque séculaire.

L'ŒUVRE ÉCONOMIQUE DE COURNOT

Avant d'examiner le détail des questions économiques traitées par Cournot, il est essentiel de préciser l'esprit dans lequel il les aborde: pour Cournot, la science des richesses doit être cultivée, comme les autres disciplines scientifiques, pour elle-même, en dehors de toute application concrète, et dans le seul intérêt de la vérité.

En se plaçant sur ce terrain, Cournot se détache nettement des économistes qui l'ont précédé, et qui tous avaient exclusivement en vue la solution de problèmes politiques, autant que spécifiquement économiques; qu'il s'agisse des mercantilistes, des physiocrates ou des libéraux, c'est toujours en hommes d'État qu'ils raisonnent, et il ne leur viendrait pas à l'esprit de cultiver la science économique en dehors de fins pratiques. Le haut idéal de Cournot a permis au contraire de conférer aux études économiques leur caractère désintéressé et leur a pour ainsi dire donné droit de cité dans le domaine de la science pure; les sincères amis de la spéculation théorique se doivent de garder envers Cournot une certaine gratitude, pour la nouvelle direction qu'il a imprimée à l'analyse de ces phénomènes.

Le plus curieux est de constater, ainsi qu'il advient fréquemment en pareille circonstance, que certaines des théories élaborées par notre auteur ont eu des applications pratiques immédiates, comme en témoigne par exemple la méthode adoptée le plus généralement, pour l'exploitation commerciale des monopoles.

Le caractère scientifique des études économiques oblige Cournot à recourir à certaines abstractions, mais il s'empresse de reconnaître qu'elles ne s'imposent pas nécessairement et présentent plus d'arbitraire que dans le domaine des sciences parvenues à un stade de développement plus avancé.

Parmi les matières traitées par Cournot, nous croyons devoir retenir exclusivement celles qui nous paraissent le plus importantes, à savoir: la loi du débit, le monopole, et le commerce extérieur.

1. *La loi du Débit.*—Pour Cournot, la loi du débit traduit la manière dont la demande effective d'une marchandise varie en fonction de son prix; il est essentiel de noter à cet égard l'attitude prise par Cournot qui considère que dans les phénomènes d'échange, l'offre et la demande ne jouent pas, en général, un rôle symétrique: la demande constitue le fait essentiel, car elle exprime pour les individus la nécessité de réaliser certaines consommations pour satisfaire à leurs divers besoins, tandis que l'offre n'est que la contrepartie obligatoire de la demande et par conséquent, le fait accessoire. Walras adoptera la même attitude, alors que d'autres économistes comme Marshall, estimeront que l'offre, conditionnée par les phénomènes de production, constitue en dernière analyse, le ressort de toute activité économique. Sans avoir l'ambition

de trancher ici le débat, nous inclinons plutôt à croire, avec Cournot, que la demande représente le phénomène essentiel, parmi les deux termes de l'équilibre économique, puisqu'en cette matière, tout se réduit en dernière analyse à des consommations soumises à la loi du débit.

Il est également essentiel, pour présenter les conceptions de Cournot de préciser dans quelles conditions varie le débit d'un article, en fonction de son prix. Bien que Cournot considère le milieu économique comme constituant un tout, dont les divers éléments réagissent nécessairement les uns sur les autres, il élabore dans sa loi du débit une conception abstraite, telle que la demande d'une marchandise ne soit fonction que d'une seule variable, son prix. Cette manière de voir ne constitue d'ailleurs, pour notre auteur, qu'une approximation, en vertu de laquelle les variations affectant le débit et le prix de tous les autres articles doivent être considérées comme du second ordre, et par conséquent négligeables vis-à-vis de celles qui concernent la marchandise étudiée. Cette présentation s'oppose à celles de Walras et de Pareto, qui font intervenir, dans leurs équations de l'équilibre économique, des fonctions de demande à plusieurs variables; cependant, Walras reconnaît que sur un marché où se rencontrent un très grand nombre d'échangistes, qui opèrent sur un très grand nombre de marchandises, il est possible de considérer des fonctions de demande dépendant exclusivement du prix de la marchandise étudiée.

Sous réserve de tenir compte des phénomènes de substitution, et de limiter en conséquence le champ de variation du prix de chaque article, la loi du débit envisagée par Cournot rend les plus grands services à la science économique et permet d'aboutir, avec une approximation suffisante, á la solution de problèmes précis, comme la théorie du monopole.

En second lieu, Cournot, faisant application de la loi des grands nombres, admet que la demande est fonction continue du prix, et il pose en outre comme règle à peu près générale, que cette fonction est décroissante; ce principe, qui reste encore admis aujourd'hui sous certaines réserves, est le point de départ de toutes les conclusions de Cournot, en matière de monopole, de concurrence, de revenu social et de commerce extérieur.

Cournot établit d'ailleurs déjà des distinctions entre les diverses marchandises selon les facultés de variation de leur débit en fonction de leur prix, et il eût certainement souscrit à la définition du coefficient d'élasticité, qui nous a été donnée par Marshall, et qu'on peut utiliser avec fruit dans toutes les études précises sur la demande.

Cournot considère également, à partir de sa loi du débit, les variations de la "valeur d'inventaire" produit du débit par le prix, et il

établit même une classification des diverses marchandises selon le sens de variation de cette quantité, en fonction de leur prix. Poussant les choses à l'extrême il prend pour base d'évaluation du revenu social, la somme des valeurs d'inventaire de toutes les marchandises et, quoiqu'il distingue par la suite ce "revenu nominal" du "revenu réel," il se rapproche par ce côté, de certains économistes du dix-septième siècle, inspirateurs du mercantilisme.

Malgré tout, il semble bien que les études récemment poursuivies en matière de demande aient rajeuni les questions relatives à la valeur d'inventaire, dont le sens de variation est, comme on le sait, en étroite corrélation avec le coefficient d'élasticité.

Notons enfin, à propos de la loi du débit et du problème de la détermination des prix, que le marché théorique envisagé par Cournot est le reflet de ses conceptions mécanistes, en ce sens qu'il y fait abstraction des résistances passives; le marché parfait reste encore aujourd'hui, un des plus beaux titres de gloire du fondateur de l'école mathématique.

2. *Le Monopole.*—Dans l'œuvre économique de Cournot, la théorie du monopole occupe une place importante, puisque notre auteur la prend comme point de départ d'un acheminement progressif, qui le conduit par degrés jusqu'à la concurrence indéfinie; nous n'avons pas ici dessein de reprendre les discussions auxquelles ce processus a donné lieu, car en dehors des problèmes relatifs au "polypole," c'est-à-dire à la concurrence limitée à un petit nombre de producteurs, problème qui semble appelé à susciter de nouvelles études et pour lequel la controverse ébauchée par Joseph Bertrand, ne semble pas encore épuisée, tout le monde est d'accord aujourd'hui, pour reconnaître qu'il y a lieu de renoncer à la méthode qui permet à Cournot de passer du monopole à la concurrence indéfinie, ces deux questions devant être traitées indépendamment l'une de l'autre.

Nous voulons simplement mettre en lumière les conclusions de Cournot, qui sont encore valables et sans lesquelles l'exploitation des monopoles n'aurait probablement pas réalisé tous les progrès observés depuis lors.

Cournot, utilisant sa loi du débit, est le premier qui ait montré l'existence du prix de monopole, c'est-à-dire, du prix qui procure le maximum de produit brut, dans le cas où l'on suppose que les frais d'exploitation sont nuls, ou de produit net, lorsqu'on admet l'existence de frais d'exploitation; Cournot s'occupe d'ailleurs principalement de l'exploitation des agents naturels, soumise à la loi du rendement décroissant et sur ce point particulier, il est bien certain que nos idées ont évolué, puisqu'à l'heure actuelle, le monopole est pratiqué pour un grand nombre d'industries élaborant des produits manufacturés à prix de revient décroissant.

Ce qu'il y a d'intéressant à retenir, c'est la simplicité, la sûreté et la précision de la méthode à laquelle il n'y a pour ainsi dire, rien à retoucher aujourd'hui. La théorie du monopole se rattache étroitement à la notion de valeur d'inventaire et Cournot observe avec beaucoup de finesse que la concurrence doit être généralement préférée au monopole, car, avec celui-ci, le principe hédonistique incite surtout les producteurs à l'accroissement du revenu nominal, et non du revenu social réel.

Il est à noter aussi que, dans ses conclusions relatives à l'exploitation des agents naturels, Cournot formule d'une manière très précise la loi du coût de production marginale telle que nous la connaissons à présent, et Marshall saura volontiers reconnaître l'influence de Cournot à cet égard.

Enfin, Cournot est le premier qui, grâce à la théorie du monopole, ait envisagé l'extension de la rente, considérée comme dûe à l'existence d'un écart non négligeable entre le prix de vente et le prix de revient; c'est lui qui, en particulier, a eu clairement conscience de phénomènes tels que celui de la rente procurée par l'exploitation d'un brevet d'invention.

Il fut sans doute moins heureux, lorsqu'il formula son principe de la compensation des demandes, qui ne constituait à ses yeux qu'une application de la théorie des erreurs, et par conséquent une approximation, faisant abstraction des variations du second ordre mais qui, par ses contradictions avec l'unité du système économique et l'interdépendance des divers éléments en jeu, devait le conduire à des conclusions manifestement discutables, au cours de ses études sur les variations du revenu social.

Retenons en tout cas le bonheur avec lequel il sut tirer parti de l'emploi des fonctions indéterminées, pour aborder avec plein succès la théorie du monopole, à laquelle son nom restera toujours attaché.

3. *Le Commerce international.*—Dans les questions relatives à la communication des marchés, Cournot utilise toujours les mêmes procédés, c'est-à-dire qu'il examine ce qui se passe pour une seule marchandise, en faisant abstraction des perturbations relatives aux autres marchandises, et en étudiant les variations du revenu nominal et du revenu réel.

Les conclusions auxquelles il aboutit tendent somme toute à préconiser, pour chaque nation, le développement des importations de produits extraits du sol, et des exportations d'objets manufacturés.

Ces conclusions sont parfaitement en accord avec les idées de Cournot sur le rôle de l'État dans la vie des sociétés car, aussi bien pour les échanges intérieurs que pour le commerce international, Cournot se montre délibérément interventionniste; c'est seulement à la fin de sa

vie, et notamment en 1877, dans la *Revue sommaire des doctrines économiques*, que, par crainte du socialisme, il prêche le libéralisme économique, estimant que le développement ininterrompu de l'interventionnisme exige une réglementation de plus en plus complexe et manifestement en opposition avec la simplicité des lois naturelles.

En dehors de la théorie du change international, dans laquelle Cournot définit très judicieusement le rôle de l'arbitrage, et pose les relations qui existent entre les coefficients de change des divers pays, l'œuvre de Cournot, en ce qui touche le commerce international, ne présente pas la même solidité que ses travaux relatifs à la loi du Débit et à la théorie du monopole; il semble bien qu'il ait prématurément appliqué l'analyse mathématique à des problèmes qui s'y prêteront sans doute un jour, mais que l'état des connaissances scientifiques ne permettait pas de traiter en son temps.

COURNOT ET SES CONTEMPORAINS

Lorsqu'on prend connaissance de la préface de la *Théorie des richesses* on ne peut s'empêcher de noter une certaine amertume en présence du peu de succès qu'obtint l'ouvrage publié en 1838, et l'on est conduit à se demander quelle fut la raison de ce discrédit. Au reste, ce ne sont pas seulement les travaux d'ordre économique de Cournot, qui furent défavorablement jugés par ses contemporains et surtout ses compatriotes. Qu'il s'agisse en effet, de ses études historiques, de ses conceptions philosophiques ou sociales, Cournot s'est heurté en ces divers domaines à une indifférence presque complète.

Depuis que les travaux de Cournot ont reconquis la place qu'ils méritaient, on est fondé à penser que l'attitude de ses contemporains tenait simplement à la nature des idées régnantes au milieu du dix-neuvième siècle.

Par sa formation intellectuelle, par la profondeur de ses idées, par la forme géométrique de ses exposés, Cournot fait nettement figure d'isolé, en un temps où la pensée philosophique était inspirée par le dialectisme et peu soucieuse de recourir aux procédés véritablement scientifiques, dont se recommandera le positivisme, qui allait bientôt conquérir tous les esprits.

Par la nouveauté de ses conceptions, et par leur hardiesse même, Cournot a certainement devancé son époque, et il devait par cela même subir la disgrâce de tous les novateurs.

Dans la préface à laquelle nous faisions allusion précédemment, Cournot exprimait modestement le voeu que ses recherches fussent plus justement appréciées, dans l'avenir, et il faut aujourd'hui se féliciter de voir ce voeu exaucé; en dehors des économistes que nous avons précédemment cités, le monde savant tout entier s'accorde à présent à

reconnaître l'influence de Cournot dans les multiples domaines où il exerça son esprit sagace; est-il témoignage plus flatteur que celui d'Henri Poincaré qui, en 1905, consacra une substantielle étude à "Cournot et les principes du calcul infinitésimal?"

Dans cette étude, où l'auteur ne dissimule pas son estime pour Cournot, sont développées maintes idées, qui nous sont aujourd'hui familières, et dont Cournot s'est fait le champion, en les enrichissant d'une contribution personnelle; telles sont les conceptions relatives à l'espace et au temps, héritées de Leibnitz, la notion de continuité, le rôle des théories scientifiques et l'usage du langage mathématique pour exprimer la simplicité des lois naturelles.

Esprit singulièrement vigoureux, apte à s'exercer dans les branches les plus diverses de la connaissance, Cournot imprime à son œuvre économique, un caractère très original, par la cohésion, c'est-à-dire par la volonté de rattacher son exposé à un petit nombre de principes très généraux. Qu'il ait parfois présumé de ses forces, et tenté de résoudre des problèmes, qui ne donnaient pas encore prise à l'analyse mathématique, rien n'est moins contestable, et ses erreurs doivent précisément nous servir à limiter nos recherches à la solution de problèmes nettement circonscrits, en attendant que les progrès de la science économique permettent d'entreprendre de plus vastes études.

De toutes les controverses auxquelles son oeuvre a donné lieu, Cournot est sorti grandi; malgré son austère pessimisme, il reste une des figures les plus attachantes de son temps; et par sa haute probité intellectuelle, un des meilleurs serviteurs qu'ait jamais connu la science, au sens le plus élevé de ce mot.

Ainsi que le souhaitait Cournot, nous pouvons donc résolument affirmer que la postérité a cassé l'arrêt rendu par ses contemporains, au bénéfice de la vérité scientifique; sachons reconnaître en Cournot un des précurseurs les plus éminents du mouvement qui se manifeste aujourd'hui dans tous les pays, en vue d'asseoir les études économiques sur des bases rigoureuses et véritablement scientifiques.[1]

L'École Nationale des Ponts-et-Chaussées

[1] Parmi les ouvrages critiques relatifs à l'œuvre de Cournot, nous croyons devoir tout particulièrement signaler: (1) *Études sur Cournot*, par Milhaud Paris: Editeur Vrin, 1927; Dans ce livre, sont rassemblés une série d'articles consacrés à l'œuvre philosophique de Cournot et publiés de 1902 à 1911. (2) *Du principe de liberté économique dans l'œuvre de Cournot et dans celle de l'École de Lausanne* (*Walras, Pareto*) Thèse de M. le capitaine Bompaire Paris: Editeur Sirey, 1931; Ce dernier ouvrage constitue un document précieux pour ceux qui s'intéressent à l'histoire de l'économie mathématique.

Nous devons également remercier notre camarade, M. Jean Cournot, répétiteur à l'École Polytechnique et professeur au Conservatoire des arts et métiers, petit-neveu d'Augustin Cournot, qui a bien voulu nous communiquer le portrait figurant en tête de cette note.

THE RÔLE OF STATISTICAL METHOD IN ECONOMIC STANDARDIZATION

By W. A. SHEWHART

Abstract of lecture presented at the University of London, May, 1932.

1. OBJECTIVE

IMAGINE living in a world without standards—no signs, no words, no symbols for things which *mean* the same to all. Imagine carrying on a business like that of the Bell System without standards. Take, for example, the telephone itself: it is not so simple as it looks. To make it requires 201 parts; and to connect it with another instrument requires approximately 110,000 other parts. The annual production of most of these parts runs into the millions, so that the total annual production of parts runs into the billions. The manufacturing organization, the Western Electric Company, buys for use in its plants more than 25,000 different items of materials and supplies, and these come from more than 15,000 suppliers located in every state in the Union.

Let us examine some of the fundamental problems involved in establishing standards of quality—standards by which the consumer may judge the quality of product, and which in themselves represent the goal of the producer—and in so doing point out the rôle of statistical methodology. We shall consider some of the problems involved in establishing what we shall term *economic standards*—those where, under the given conditions in respect to the development of science and the development of human wants, there is a balance between the economic value to the consumer of any possible modification in the quality standard and the cost of such modification. To begin, let us consider the question:

2. WHAT IS A STANDARD?

I have before me as I write this paragraph a book of several hundred pages published by the American Society for Testing Materials. Its title is *A.S.T.M. Standards, Part I, Metals, 1930.* As is true of almost every other book, the only things I find inside are some symbols, lines, and pictures. These are supposed to specify some of the things which consumers want and producers are to make. It is essential therefore that they *mean* the same to both.

But when do symbols mean the same to a given group of people? The modern logician will likely answer that a symbolic statement means the same to two or more persons when it leads to the *same action* on the part of each of them.

If I were to ask the question:

$$2 + 2 = ?,$$

all of us would answer 4. If an officer of the law were to enter and say—"Leave this room immediately"—there would be a likeness in our actions. If, however, each of us were to try to make on a piece of paper ten straight lines of the same length, it is almost certain that the lines would not be identical. Now, go one step further, and imagine specifying such a simple thing as a wire nail so that the action on the part of every one of several producers would be the same.

There are at least two ways used by engineers to determine when two or more things are the same. One is for those interested to agree that they are the same for all intents and purposes. The other is for those interested to agree to a method of measuring the things, thereby getting as many sets of numbers as there are things, and further, to agree to a method of comparing the sets of numbers so obtained. Note that in either case the determination is simply an agreement. To settle upon a method of measurement again introduces the difficulty of determining when the method agreed upon—necessarily symbolic in form—means the same to all.

Thus the specification of a standard is of value only in so far as it indicates a way of arriving at an empirical decision acceptable to both producer and consumer as to whether or not a given thing is to be considered as being of standard quality. Only through agreement either upon the human judge or upon the formal processes of analysis of quantitative data can we attain this acceptable decision.

As a specific illustration, let us consider the standard of length—perhaps the simplest in existence. There is the concept, corresponding to the essence of the thing itself and represented customarily by the symbol L. Associated with this, there is that of the quantity of length involving a conceptual method of measurement. In other words, we think of so much of something—in this case so much of that which we designate L.

Suppose we have a number n of things whose lengths are to be measured such, for example, as straight lines. First we conceive of these lines having lengths measurable by some ideal process such that the degrees of so-muchness of the lengths are in point of principle expressible by numbers

$$X_1', X_2', \cdots, X_i', \cdots, X_n', \tag{1}$$

in terms of the chosen unit of length.

The actual process of measuring these same lines gives us, however, a corresponding set of numbers,

$$X_1, X_2, \cdots, X_i, \cdots, X_n. \tag{2}$$

Fundamentally (1) is conceived to exist, but we do not know the numbers; (2) is known and represents the results of applying a given rule of procedure of practical measurement for establishing such a set of numbers. Set (2) is that which we use as a basis for inferring something about the unknown set (1).

Suppose that we take the length of one of the lines as the standard of comparison and ask if the others are to be considered as being of this standard length. The only thing that we can do is to try to answer this question from a consideration of the set of numbers (2), no one of which may represent the true length of the corresponding line. Obviously to get any place, we must agree to a method of interpreting the data. This example shows that we cannot expect the specification of a standard to outline a way of determining whether or not a thing *is* of standard quality. This is true even when what we mean by quality is limited to a few measurable characteristics, for we always have to allow for errors of observation. To the question, *Is* a given thing of standard quality? there can be no positive answer, yes or no. This is simply another way of saying that all inference is of the nature of probability or degree of belief inference.

This situation is of far reaching importance in the problem of standardization. There is always a certain amount of variability which must be left to chance. The economics of the situation gives a basis for arriving at a definite answer to the question—How much variability shall be left to chance?

The postulational basis for answering this question is: A variation should be left to chance only when produced by a constant system of chance or unknown causes among which no single cause or distinguishable group of causes appears to have a predominating effect. This means, of course, that the practical test for constancy of this type is an agreed-upon criterion, or set of criteria, based upon *statistical* theory and particularly on the notion of statistical stability.

3. The Notion of Statistical Stability

Perhaps the simplest observable type of statistical stability is that defined as follows: Whenever an event may happen in only one of two ways, and the event is observed to happen under the same essential conditions for a large number of times, the ratio p of the number of times the event happens in one way to the total number of trials appears to approach a definite limit, let us say p', as the number n of trials increases indefinitely. Here p' is taken to be the statistical probability. Symbolically we may state this Law of Large Numbers in the form

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$$\lim_{n \to \infty} p = p', \tag{3}$$

where L_s stands for the statistical limit which differs from a mathematical one in that we do *not* reach a number n_0 of trials such that, for all values of n greater than n_0, the ratio p will differ from some fixed value p' by less than some fixed positive value ϵ.

However, this formal definition is meaningless until we go further and consider how in practice we may put meaning into the phrases "an essential condition," and "statistical limit." We shall try to do this by a series of examples.

A typical case in point is that of tossing a coin and observing the successive values of the ratio p of the number of heads to the total number n of throws as the number n is increased indefinitely, as is illustrated by the results of one thousand throws shown in Fig. 1. I

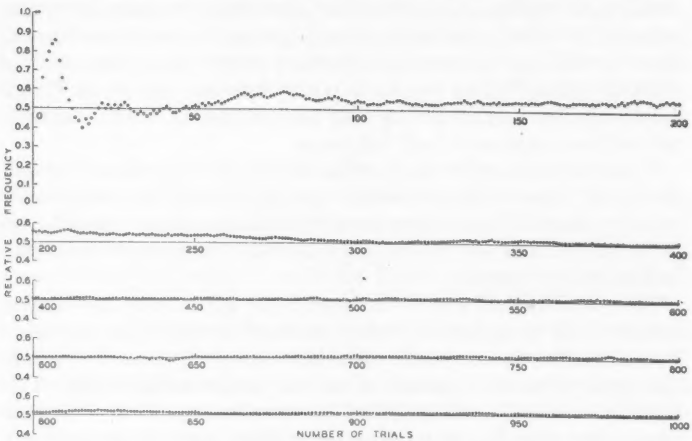

FIGURE 1. AN EXAMPLE OF STATISTICAL CONSTANCY.

have discussed in some detail elsewhere[1] several of the important characteristics of this limit of interest to engineers. For our present purpose, it is sufficient to call attention to the fact that formal mathematical criteria exist for assisting in determining when a set of data gives evidence of this kind of statistical constancy, and that these criteria have been satisfied in cases coming to our attention where those interested have been in a place to agree that the same essential conditions had been maintained.

[1] W. A. Shewhart, *Economic Control of Quality of Manufactured Product*, New York: D. Van Nostrand Company, 1931.

As another example, let us consider the objective charge X' on an electron. It is generally assumed that this is a constant. A series of measurements of this charge, however, do not show constancy when taken even by an outstanding physicist like Millikan. In fact our only approach to the assumed objective reality is statistical. All we can say is that the average of a number n of measurements appears to approach a statistical limit. Actually, successive averages of 1, 2, 3, . . . of Millikan's measurements form a sequence of much the same kind as the one pictured in Fig. 1.

But in the present era of physical science there are very few instances where we even assume, as in the electron example, an objective constancy on the part of measurable characteristics. Instead we assume that the objective thing measured is itself statistical in nature. Witness, for example, any physical property such as density. Fig. 2 shows the

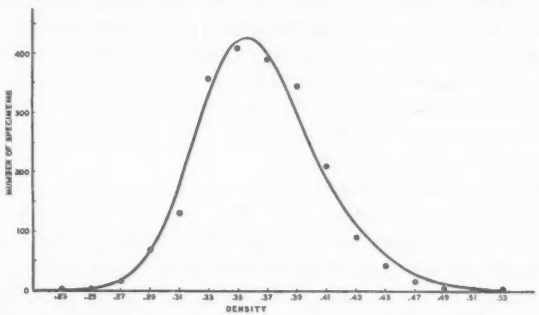

FIGURE 2. EXAMPLE OF STATISTICAL NATURE OF PHYSICAL PROPERTIES.

observed distribution of density determined on 2,105 small clear specimens of Sitka spruce, the wood at one time used extensively in making airplane propellers.[2]

In this case, care had been taken to eliminate causes of variability by careful selection of experienced engineers. Yet we note the wide variability in the measurements. Of course, this is partly attributable to errors of observation but, in this case, the error effect is small. Our best guess as to the character of the objective frequency function is represented by the solid curve.

As yet another example, consider a *Functional or Mathematical Law or Relationship:* One of the best known specifications of this type is

[2] L. J. Markwardt, "Comparative Strength Properties of Woods Grown in the United States," *Technical Bulletin No. 158, U. S. Dept. of Agriculture*, February, 1930.

the equation of state of a perfect gas, $PV = RT$, where P is the pressure; V, the volume; T, the temperature; and R the gas constant.

Taking the simplest case of two variables Y' and X', let us represent the assumed objective constancy by the functional relationship,

$$f'(Y', X, \alpha_1', \alpha_2', \cdots, \alpha_i', \cdots, \alpha_m') = 0,$$

where the α''s are unknown parameters.

In practice what we really observe is that whenever our measurement X of the independent value X' takes on a particular value, let us say X_1, the ratio p of the number of times that the corresponding value Y will be found within any limits Y_{11} to Y_{12} to the total number of trials satisfies the statistical limit (3).

Similarly we may consider a *Statistical Relationship:* Two variables are said to be statistically related under the conditions:

(a) That the observed fraction p of the number of times that simultaneously observed pairs of values of these two variables fall within a given area in the plane representing these two variables, approaches as a statistical limit L_s some fixed value p', as the number n of observations approaches infinity, or in other words that we have (3).

(b) That the frequency distributions in the array of one variable are not the same for all of the arrays.

The great majority of relationships used as a basis for specifications are perhaps *Empirical Relationships.* This type of relationship may be exemplified by the assumed objective relationship between two physical properties of a metal. What we said about the statistical limit in our discussion of the functional relationship applies here. The only difference between this kind of empirical relationship—as we shall consider it here—and the functional relationship is that it is generally too complicated to express in a practical functional form.

We may conclude that: *Although we may conceive of various kinds of constancy as a basis for specifications of standards, the only kind of observable constancy is statistical.*

It is obvious, therefore, that the statistical nature of observable data must be taken into account both from a design and an inspection viewpoint. That is to say, the design engineer must be given information that will permit him to allow in a rational way for variability which must be left to chance. The inspection engineer needs to use statistical criteria to determine when the variability is greater than it is economical to leave to chance.

4. The Statistical Aspects of Human Wants

Let us ask a very simple question: What are the economic standard lengths of wire nails? It is conceivable that nails of every length be-

tween zero and some fixed upper limit have economic value: but needless to say, it is not feasible to manufacture nails of every length within this range.

History shows that the present commercial sizes of nails just came into existence—*laissez faire*, the economist would say. In other words, the present choice of sizes represents an agreement between consuming and producing agencies. It is sometimes argued that the practice of making nails of such sizes is of so long standing and has led to so many associated practices that it is quite reasonable to believe that these sizes may be accepted as economic standards. Is this conclusion justified? Possibly it is in this and some similar instances. Nevertheless it is not difficult to point to standards which came into existence in somewhat the same way and which later were found to be uneconomical. Witness, for example, the systems of weights and measures, many of which cannot be said to have been chosen with due consideration to the economic consequences of the choice.

One thing is certain, engineers today are in many instances giving very careful consideration to the choice of sizes when establishing new standards as, for example, in the assignment of wave lengths to broadcasting stations. The history of such assignments reveals less of the method of *laissez faire* and more of national and international planning in which an attempt has been made to weigh carefully the economic consequences of the assignments. In fact, we have today many national and international standardization organizations giving careful consideration to choice of sizes of quality characteristics of one kind or another.

Looked at closely, we see that in all cases the choice of economic standard sizes involves the problem of measuring the relation of *consumer demand* to the choice of the system of sizes. Needless to say, this involves a sampling problem of basic importance, and it is reasonable to believe that few systems of sizes existing today would bear close scrutiny from this viewpoint. In this we have a fundamental economic problem, statistical in nature, which must be given careful consideration as a part of any attempt in either national or international planning having to do with the establishment of sizes or aimed-at values of quality characteristics that will give the greatest feasible satisfaction to the maximum number of people.

My attention was first drawn to the sampling problem involved in determining human wants in connection with the specification of sizes of sound-proof aviation helmets. A similar problem is the determination of the best dimensions of a telephone handset.

In determining sizes and allowable tolerances, one of the statistical factors of interest is the distribution of the minimum detectable incre-

ment ΔX in a given characteristic for a homogeneous group of people.[3] If we vary two or more characteristics detectable by the senses, such as frequency and intensity, the objective distribution is one in two or more dimensions.

Most frequently sensation measures are of this complex sort. Take for example the noise current of a telephone circuit. I have discussed elsewhere[4] how the control of such a phenomenon requires the calibration of a machine method of measurement of the current which must be correlated to the ear measure of a homogeneous group.

The same kinds of problems are involved in color testing and in any other case where a direct sensation is at the basis of our judgment of whether or not we like or want a thing.

Needless to say, how much we want a thing depends upon what we think we know about it, and about potential things of its kind that might be made. A Robinson Crusoe would no doubt be happy with an old-fashioned radio, but his relatives must have the latest model; and the radio engineer must have something even better in his home.

But what makes each one want what he wants? There's the rub. *To determine the causal effect of a measurable characteristic of reality on our want for a thing is most assuredly a sampling problem in which experiments must be carefully planned to get the correlation between corresponding variates as high as possible so as to reduce the sampling error to a minimum.*

5. Efficient Production Method

Assuming that the end requirements can be specified in a manner agreed upon by producer and consumer, let us next consider the statistical aspects of establishing an efficient production method.

Some of the problems[5] involved are: The determination of ways and means of effecting economies through plant location, design, choice of material, purchasing, and marketing; the economic balancing of labor and machines; economic consideration in selection and purchase of equipment; efficient use of capital investment in equipment; and economic materials handling, to mention only a few.

If we examine almost any one of these problems in detail, it is but natural that we should find that the sought-for solution depends upon both physical and economic factors. The industrialist charged with the responsibility of making scientific developments of greatest use to the maximum number of people finds that the equations involving the cost

[3] W. A. Shewhart, "Some Applications of Statistical Methods," *Bell System Technical Journal*, January, 1924.

[4] *Economic Control of Quality of Manufactured Product, loc. cit.*

[5] Cf. Eidmann's *Economic Control of Engineering and Manufacturing*, McGraw Hill Publishing Company, 1931.

factor which he wishes to minimize usually, if not always, contain these two kinds of elements.

Two Bell Telephone System colleagues, S. L. Andrew[6] and R. W. Burgess,[7] both actively engaged in the application of statistical methodology to the economic aspect of these problems, have recently discussed better than I can the statistical nature of the problems involved and the need for better data—data that have a common and well defined meaning—to be used in the available statistical machinery.

What is needed most perhaps is a changed viewpoint in the application of statistics to these economic problems—*away from the summarizing objective in the form of indexes whose physical meaning is indeterminate and toward the separation of data into rational subgroups with the assistance of the latest improvements in statistical criteria.*

Turning next to the physical side of the problem, we find one of the important fields of application of statistical theory in the establishment of distribution of tolerances on piece parts or quality characteristics to come within the overall allowable tolerances. There are two types of design from this viewpoint: one in which the overall quality is a function of the qualities of component parts in such a way that the resultant deviation is of an additive kind; the other in which the resultant deviation is of a chain type in respect to the component parts.

Let us consider a very simple problem of the first type. Suppose that you are building a rack to support a load consisting of the combined weights of a number of different pieces of apparatus. Assume that past experience is available to estimate the average and the standard deviation in the weights of each of the kinds of apparatus. One very customary method in such instances is to allow for the maximum load plus a certain safety factor where the maximum load is taken as a sum of the maximum weights that have ever been observed. Of course the chance of obtaining this maximum load is negligibly small, and there is little engineering justification for designing for such a condition because the assurance attained in this instance is out of all proportion usually to the assurance attained at other points in the system. To take a simple case where the standard deviations in all the weights are equal, the satisfactory maximum load is such that the amount added to allow for dispersions in the separate weights need be only $1/\sqrt{n}$ times as large as that given by the customary method. In other words, the

[6] "The Methods of Industrial and Business Forecasting," S. L. Andrew, Chief Statistician of the American Telephone and Telegraph Company, *Bell Telephone Quarterly*, January, 1931.

[7] "A Statistical Approach to Mathematical Formulation of Demand-Supply-Price Relationship," R. W. Burgess, Chief Statistician of the Western Electric Company, *The Annals of Mathematical Statistics*, February, 1932.

method often used gives $\sqrt{n}$ times the necessary addition in strength with its associated cost.

Now, let us consider the second type of problem, or that of specifying the distribution of the breaking strength of a chain composed of m links, the breaking strengths of the links used in the chain being assumed to be normally distributed about a specified value $\overline{X}'$ with a standard deviation σ'. Obviously the more links there are in the chain, the lower becomes the expected breaking strength of the chain. The objective standard deviation also decreases. The work of Tippett[8] makes it possible for the design engineer to estimate this effect accurately.

To make these applications possible in a given design it is of course necessary that the qualities of separate piece parts be *statistically controlled* in the process of production. R. L. Jones[9] was one of the first to grasp the significance of the condition of statistical control as a basis for inspection engineering, not only because it leads to maximum consumer protection, but also because it is the logical basis for inspection procedures to attain the following five objectives: (a) To determine for each step the economic percentage rejection, or tolerance p' for defectives; (b) To reject defective material and parts at such points in the chain of production as will make the net cost of rejection a minimum; (c) To determine for each step the minimum amount of inspection which will suffice to give economic control of quality; (d) To detect failures of desired quality control as evidenced by fluctuations and trends; (e) To discover the causes of such fluctuations and trends in order to secure improved control.

The first object is, as it were, to insure that the quality of material passed from one stage to the next is as good as we ought to make it, all the costs both before and after this stage being taken into account. The tolerance for defects under these conditions will depend on at least three essential factors: (1) the cost of refining the earlier processes, as by improved machinery or more skillful personnel, (2) the cost of inspection to eliminate difficulties at a given stage, and (3) the cost of remedying defects at a subsequent stage. Stated in another way, the requirement amounts to saying that we should not let the observed fraction p defective fluctuate more than it is economical to leave to chance. But this is the condition of statistical control for economy of

[8] "On the Extreme Individuals and the Range of Samples Taken from a Normal Population," *Biometrika*, Vol. XVII, December, 1925, pp. 364–388. These tables are also reproduced in the second volume of Pearson's *Tables for Statisticians and Biometricians*, 1932.

[9] Director of Apparatus Development of the Bell Telephone Laboratories, "Quality of Telephone Materials," *Bell Telephone Quarterly*, Vol. VI, pp. 32–46.

production. Moreover the observed fraction defective at a given stage does not give a very good picture of the portion of the material that is not sampled unless assignable causes have been weeded out in previous stages. Thus from all angles it appears that the first step essentially requires that the product be statistically controlled in order to attain the first objective.

If a quality of a product is controlled in this sense the probabilities of getting 0, 1, 2, $\cdots$, n non-conforming pieces in a lot of n pieces are obviously given by the terms of the point binomial $(q'+p')^n$. The practical problem of inspection is therefore that of watching the observed fractions $p_1, p_2, \cdots, p_m$ say, as determined from samples of $n_1, n_2, \cdots, n_m$, to see if they give any indication of the existence of assignable causes of variation. From this viewpoint the emphasis in the inspection specification should be on the collection of data separated so as to detect assignable causes, if they exist, more than on the inspection of a fixed portion of the product as is often done. The best results will naturally be obtained by the engineer who is most successful in dividing his data into objective rational subgroups.

As soon as the assignable causes have been detected and removed one after another, we may expect oscillations of the fraction p similar to those in the observed ratio of heads to total number of throws of a coin. In other words, starting with the observed fraction in a sample of a single piece of product it will be either 0 or 1 and will then oscillate about some fixed value much in the same manner as the frequency in Fig. 1.

For reasons which we have already considered, so soon as we have secured statistical control we have maximized the amount of assurance given by a sample; and we are in a position to say in most instances that the rate of increase in the precision of our results is roughly proportional to the square root of the number of observations. Thus objective (c) is attained. Obviously, in attaining the state of statistical control, we have attained objectives (d) and (e).

An example will illustrate the need for statistical analysis of *variability* in problems of the kind here outlined.

At a symposium on specifications for physical properties of malleable iron castings held under the auspices of the American Society for Testing Materials, upwards of 20,000 test results[10] were contributed by seventeen different investigators.

Confining our attention to tensile strength it is of interest to note the graphical presentation of the ranges, Fig. 3, (the original test results not being available, we must make use of the ranges).

[10] Cf. Proceedings of the American Society for Testing Materials for 1931.

On one hand, let us assume that the inherent cost per casting is the same for each of the 17 sources. If so, why not raise the specification requirement from 50000 lb. per sq. in. (as specified in A.S.T.M. Designation A47-30) to some 54 or 55000 lb. per sq. in.? Why use source number 1 if material from number 5 is just as cheap? On the other hand, assume, as is more likely to be the case, that the inherent costs are not the same. Then for a given job there is a most economical form of the material, the specification for which is the economic standard. Enough has been said to indicate that the differences in the seventeen

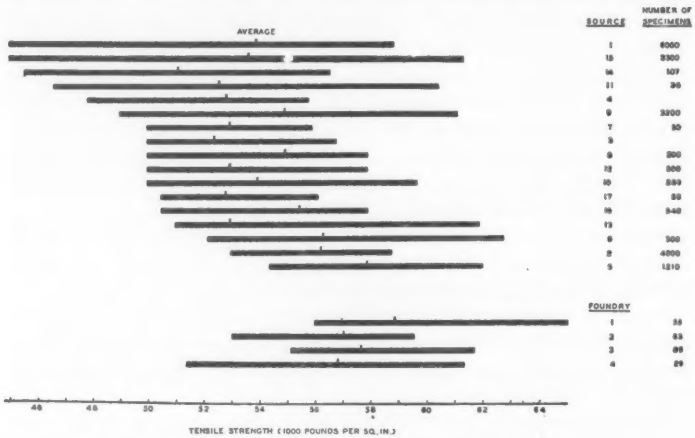

FIGURE 3. RANGES OF TENSILE STRENGTH FROM 22 SOURCES.

sets of data are likely greater than should be left to chance. *What is needed is a practical test for significant differences of this character—just what statistical theory provides.*

Only after discovering the assignable causes of the significant differences are we in a place to say what variability it is economical to accept in the standard, for only then do we know how much it will cost to take it out so that we can compare this cost with the increased value of the product resulting from the removal.

6. ECONOMETRICS AND INDUSTRIAL STANDARDIZATION[11]

In the preceding discussion an economic standard is conceived as a standard such that under the given conditions in respect to the development of science and the development of human wants, there is a balance between the economic value to the consumer of any possible

[11] This section was added September, 1932.

modification in the standard and the cost of such modification. This point of view is consistent with the vision of science in which there is not so much a struggle between men, or companies, or nations, for a limited store, where one's gain must be another's loss, as there is co-operation in an effort to raise the standards of living of all by making use of the results of progress in pure science.

It is obvious that in this effort there is need for the development of a *quantitative economic theory* which will take into account not only demand and supply but also quality. By nature the engineer is one who likes to set a goal and then try to attain it. But to do this, it is necessary for him to have available quantitative equations of economic theory in the same sense that he has such equations in the natural sciences—hence the significance of scientific economics, as supported by the Econometric Society, to the industrial leaders charged with making use of physical laws and properties in satisfying human wants.

Bell Telephone Laboratories

L'UTILISATION DES ÉQUATIONS FONCTIONNELLES ET DES NOMBRES COMPLEXES DANS LES RECHERCHES ÉCONOMIQUES

PAR J. TINBERGEN

Communication présentée à la réunion de la Société d'Économétrie, Lausanne, septembre 1931.

1. INTRODUCTION

LES considérations suivantes se rapportent à la théorie des oscillations économiques. Comme résultat idéal de toutes les recherches à ce sujet, on peut, il me semble, indiquer la découverte des causes et du mécanisme du mouvement cyclique général des affaires. Au point de vue quantitatif, on est encore loin d'avoir satisfait à cette exigence idéale. Un accord satisfaisant entre théorie et observation statistique n'existe que dans certains cas très simples. Pour les cas plus compliqués, il n'y a guère de théorie quantitative suffisamment élaborée, et les corrélations qu'on a trouvées expérimentalement manquent souvent d'une base logique assez fondée. Il y a donc ici un champ très étendu pour la Société d'Économétrie. J'espère donner ici une petite contribution aux recherches théoriques dans ce champ. Cette contribution n'a cependant qu'un caractère préparatoire.

Comme presque toute considération théorique part de certaines approximations de la réalité, l'étude suivante est basée elle aussi sur quelques hypothèses simplificatrices. Ces simplifications sont suggérées spécialement par les recherches de M. Hanau sur la formation des prix du porc[1], montrant qu'il y a des cycles très nets d'environ 3 à 4 ans dans les prix et l'offre de cet espèce de viande. Ces cycles (les cycles "porcins") ont pour cause principale le délai entre le prix et l'offre; délai résultant surtout de la durée de la production. De pareils cycles avaient été démontrés antérieurement par d'autres auteurs, mais il me semble que les recherches de M. Hanau sont les plus complètes et les plus nettes. La théorie de ces cycles a été développée par M. Moore.[2]— L'existence d'un délai assez constant entre le prix et l'offre a été démontrée pour plusieurs autres biens.[3] A côté de l'hypothèse d'un délai constant je me baserai sur l'hypothèse que les courbes d'offre et

[1] A. Hanau, Die Prognose der Schweinepreise. Vierteljahrshefte zur Konjunkturforschung, Sonderheft 18. Berlin, 1930.

[2] Voir p.e. H. L. Moore, *Synthetic Economics*, New York, 1930.

[3] Voir p.e. M. Ezekiel, Preisvoraussage bei landwirtsch. Erzeugnissen, Veröffentlichungen der Frankfurter Gesellschaft für Konjunkturforschung, Heft 9 Bonn, 1930.

de demande soient rectilignes. La plupart des recherches sur ce point me semblent indiquer que cette hypothèse est valable avec assez d'exactitude. En troisième lieu je me base sur quelques recherches sur la spéculation par le Département d'Agriculture des États-Unis, montrant que la demande spéculative (voir §3) est à peu près proportionnelle à la dérivée du prix par rapport au temps.

Les problèmes théoriques, que je traiterai, partent du cas idéal qu'on peut construire des recherches de M. Hanau, le cas le plus simple, en même temps, des "Synthetic Economics" de Moore, et le généralisent de plusieurs manières. Une telle généralisation est nécessaire quand on désire approximer successivement les mouvements cycliques réels. D'un côté la forme extérieure de ces mouvements diffère essentiellement de celle des cycles "porcins." Dans ces cycles-là la différence de phase entre le prix et l'activité (voir §5) est d'$\frac{1}{4}$ de la période, tandis que cette différence de phase est très petite pour les cycles généraux: c'est là justement la cause que dans la courbe B du baromètre de Harvard University les prix et l'activité ont été combinés. D'autre part le caractère du marché du porc est beaucoup trop simple ce qui explique peut-être les différences signalées. Ce caractère trop simple consiste dans les approximations suivantes valables pour le cas du porc:

1. le bien considéré a une période très courte de consommation;
2. la demande dépend principalement du prix; d'autres facteurs, comme le pouvoir d'achat des consommateurs n'ont qu'une influence secondaire;
3. il n'y a pas de spéculation à ce marché.

Les généralisations désirables seront donc:

1. La période de consommation n'est pas négligeable;
2. La demande dépend encore d'autres facteurs. Surtout me semble intéressant le cas où le pouvoir d'achat a une influence considérable et où ce pouvoir d'achat résulte de l'activité dans la branche elle-même. C'est ce qu'on aura aussitôt qu'on considère un système économique fermé; pour une théorie endogène des cycles un tel système doit être indispensablement la base. En second lieu il sera intéressant de traiter le cas où existe un plus grand nombre de marchés et qui s'influencent réciproquement.
3. Une demande spéculative est introduite. Naturellement le problème des cycles économiques n'est pas épuisé par ces généralisations. Le présent auteur ne l'ignore pas du tout. Une première restriction très grande est que seulement des mouvements périodiques seront contemplés. Beaucoup d'autres restrictions pourraient être nommées. L'importance de l'étude suivante est donc très bornée.

Le fait remarquable du point de vue mathématique c'est que les

problèmes posés—problèmes d'une nature économique très simple—nous mènent déjà à des questions mathématiques assez compliquées.

2. Bien de consommation directe

Les propriétés caractéristiques, pour notre but, du marché d'un tel bien seront retrouvés dans le schéma suivant:

$$\left.\begin{aligned}&\text{Fonction de l'offre: } A_0 + A_1 p(t-\theta);\\ &\text{Fonction de la demande: } B_0 - B_1 p(t),\end{aligned}\right\} \tag{1}$$

ou A_0, A_1, B_0, et B_1, sont des constantes positives, $p(t)$ est l'écart entre le prix au moment t et le prix P d'équilibre à la longue, et θ est le délai entre le prix et l'offre (p.e. la durée de la production). De la définition de $p(t)$ il suit que

$$A_0 = B_0,$$

tandis qu'à chaque moment on aura l'égalité de l'offre et de la demande instantanés:

$$A_1 p(t-\theta) = -B_1 p(t), \tag{2}$$

équation à laquelle doit satisfaire le mouvement de p. On voit que, étant donnée la valeur de $p(t)$ pour l'intervalle

$$0 \leqq t < \theta, \tag{3}$$

le mouvement de $p(t)$ pour tout $t \geqq \theta$ sera connu.

L'équation (2), équation fonctionnelle linéaire de type simple, peut être résolue d'une façon très facile. On aura

$$p(t) = -\frac{A_1}{B_1} p(t-\theta),$$

d'où l'on déduit sans peine que le mouvement du prix sera une oscillation purement périodique pour $A_1 = B_1$, une oscillation amortie pour $A_1 < B_1$ et une oscillation "explosive" pour $A_1 > B_1$. La forme de l'oscillation dépend entièrement du mouvement donné du prix pour l'intervalle (3), tandis que sa période T est en tout cas $T = 2\theta$.

Bien que cette solution soit tout à fait claire, il sera utile d'établir encore une autre méthode de solution, méthode qui peut nous aider dans des cas plus compliqués.

Posons

$$p(t) = Cz^t \tag{4}$$

où C et z sont des constantes quelconques.

L'équation (2) deviendra:

$$A_1 C z^{t-\theta} + B_1 C z^t = 0,$$

d'où l'on déduit que C est arbitraire, tandis que

$$A_1 z^{-\theta} + B_1 = 0.$$

On voit que seulement des z complexes satisferont à cette équation; l'argument de z peut être;

$$\frac{\pi}{\theta}, \frac{3\pi}{\theta}, \frac{5\pi}{\theta}, \text{ etc.}, \tag{5}$$

et

$$\frac{-\pi}{\theta}, \frac{-3\pi}{\theta}, \frac{-5\pi}{\theta}, \text{ etc.} \tag{6}$$

Comme dans cette étude nous nous bornerons aux cas où le mouvement est strictement périodique, cela signifie que, pour le problème sous considération, nous discuterons seulement le cas où $A_1 = B_1$. Il suit que z^t sera une fonction purement périodique, dont la période T peut être

$$2\theta, \tfrac{2}{3}\theta, \tfrac{2}{5}\theta, \cdots \text{etc.}$$

En combinant des solutions (5) et (6) ayant une même valeur absolue de l'argument, on peut obtenir des solutions réelles, chose nécessaire pour une solution avec un sens économique.

L'équation (2) étant linéaire on voit sans peine que la solution réelle générale peut s'écrire dans la forme:

$$p(t) = \sum_0^\infty{}^k C_{2k+1} \cos\left[\frac{(2k+1)\pi t}{\theta} + \epsilon_{2k+1}\right],$$

où C_{2k+1} et ϵ_{2k+1} sont des constantes; cela veut dire que $p(t)$ sera une fonction périodique de la période 2θ, résultat concordant avec notre première solution.

3. Introduction de "spéculation simple"

Etudions maintenant l'influence d'une certaine forme de spéculation en introduisant à côté de la demande statique (1) une "demande spéculative." Cette demande spéculative peut, a priori, dépendre de manières très différentes du prix. Il me semble qu'il faut faire distinction entre plusieurs formes de spéculation au fur et à mesure que

(a) le mouvement du prix est connu ou inconnu;

(b) l'horizon économique[4] est court ou long; et

(c) le coût de l'emmagasinage est bas ou élevé. Il n'est pas l'intention de l'article présent de traiter ces questions; nous supposerons seulement

[4] C.à.d. la période pour laquelle le sujet économique fait ses projets.

que la "demande spéculative" soit de la forme suivante, que nous caractériserons par l'indication, "spéculation simple":

$$B_2\dot{p}(t) \equiv B_2\frac{dp}{dt}.$$

Les recherches du Department of Agriculture des États-Unis sur la spéculation sur le blé ont démontré que dans le cas des "petits spéculateurs" cette forme est une bonne approximation. La demande totale devient alors

$$B_0 - B_1p(t) + B_2\dot{p}(t),$$

forme introduite aussi par Amoroso[5] et par Roos et Evans.[6]

En utilisant maintenant la deuxième méthode de solution établie dans le §2, nous posons:

$$p = Cz^t.$$

On aura:

$$\dot{p} = Cz^t \log z,$$

et l'équation exprimant l'égalité de l'offre et de la demande devient

$$C(A_1z^{t-\theta} + B_1z^t - B_2z^t \cdot \log z) = 0.$$

On en déduit que C est arbitraire et que z doit obéir à l'équation:

$$A_1z^{-\theta} + B_1 - B_2 \log z = 0. \tag{7}$$

Il sera possible sans peine de trouver des solutions de cette équation. Pour une combinaison concrète quelconque de valeurs d'A_1, B_1, et B_2, une série infinie de solutions peut être indiquée, analoguement au cas (5), (6), avec des arguments montants. Nous appellerons solution "d'argument minimum" la solution dans une telle série dont l'argument a la valeur la moins élevée. Pour simplifier les discussions nous ne nous occuperons que des solutions,

$$\left.\begin{array}{ll} (1) & \text{"d'argument minimum."} \\ (2) & \text{dont } |z| = 1. \end{array}\right\} \tag{8}$$

La justification de ces restrictions est la suivante:

(1) dans plusieurs cas pratiques il se montre que les solutions d'argument minimum sont, du point de vue économique, les plus im-

[5] Voir p.e. L. Amoroso, *Giornale degli Economisti* 1930, p 941.

[6] Voir p.e. G. C. Evans, The Dynamics of Monopoly, *Amer. Math. Monthly*, 1924, (XXXI). C. F. Roos, A Mathematical Theory of Competition, *Amer. J. of Math.*, 1925, (XLVII).

portantes; et (2) les solutions où $|z|$ diffère sensiblement de l'unité représentent des oscillations soit fortement amorties soit "explosives," qui dans la réalité ne sont pas encore observées très nettement. L'importance théorique de ces cas-là n'est pourtant pas du tout niée par le présent auteur.

La coïncidence des deux restrictions (8) exige déjà une relation entre A_1, B_1, et B_2; leur vrai sens est donc que nous ne considérons non seulement qu'une partie des solutions mais aussi qu'une partie des problèmes possibles.

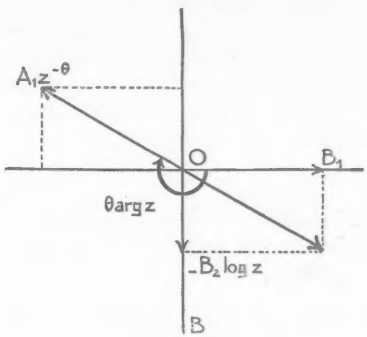

FIG. 1

La représentation graphique dans la Fig. 1 nous éclaircira la situation dans les problèmes en considération. Pour les nombres complexes on a:

$$\log z = \log|z| + i \arg z, \qquad i = \sqrt{-1}$$

ce qui devient dans notre cas

$$\log z = i \arg z.$$

Pour un argument positif de z, le terme $-B_2 \log z$ dans l'équation (7) sera représentée par un vecteur du sens OB, le terme $+B_1$ par le vecteur OB_1. Il suit que le vecteur représentant $A_1 z^{-\theta}$ sera situé dans le second quadrant, d'où l'on tire la conclusion que

$$\pi < \theta \arg z < \frac{3\pi}{2},$$

et, par suite,

$$\frac{4}{3}\theta < T < 2\theta, \tag{9}$$

T étant la période, de telle façon que, quand la spéculation simple

est faible (c.à.d. B_2 petit), T diffère peu de 2θ et que l'augmentation d'intensité de cette spéculation diminue la valeur de T.

Les inégalités (9) ne sont pas seulement valides pour z, mais aussi pour les solutions réelles pour $p(t)$ qui y correspondent. On arrive à la proposition suivante: *l'introduction de spéculation simple raccourcit la période de l'oscillation.*

Il me semble être intéressant de rappeler la circonstance que la présence seule de la spéculation simple (sans qu'il y ait un délai entre le prix et l'offre) ne suffit pas pour générer des oscillations.

Remarque. On est mené à une équation de type similaire quand on entend par $A_0+A_1p(t-\theta)$ la quantité produite, par $B_0-B_1p(t)$ la quantité consommée d'un certain bien et quand on remplace la condition de l'égalité de ces deux quantités par une autre condition d'équilibre, à savoir que le stock du bien en considération est une fonction linéaire de $p(t)$, disons

$$-B_2p(t),$$

où B_2 est positif d'après les expériences dont on dispose. De cette dernière condition on conclut sans peine que l'augmentation du stock au moment t,

$$A_0 + A_1p(t-\theta) - B_0 + B_1p(t) = -B_2\dot{p}(t);$$

on a donc, puisque $A_0=B_0$,

$$A_1p(t-\theta) + B_1p(t) + B_2\dot{p}(t) = 0.$$

En posant $p(t)=Cz^t$ on obtient

$$A_1z^{-\theta} + B_1 + B_2 \log z = 0, \tag{10}$$

équation ne différant de (7) que par le signe du dernier terme. On en déduit que maintenant

$$\frac{\pi}{2} < \theta \arg z < \pi,$$

c.à.d. que $2\theta < T < 4\theta$.

En appelant $1/B_2$ la "réagibilité du prix par rapport au stock" on voit donc:

(1) que dans le cas où quantité produite et quantité consommée sont toujours égales, cette réagibilité est infiniment grande, chose qui empêche toute oscillation des stocks; et (2) qu'une diminution de cette réagibilité tend à allonger la période d'oscillation des prix.

4. Demande hyperbolique

Comme introduction d'un cas plus compliqué il sera utile de considérer encore une fois le cas premier, sans spéculation, changé un petit peu par l'introduction d'une fonction de demande de la forme:

$$\frac{C}{P + p(t)}, \tag{11}$$

où C est une constante et le dénominateur représente le prix du bien considéré. On pourrait appeler C le pouvoir d'achat des consommateurs, en supposant que le bien en considération est le seul bien acheté.

L'égalité de l'offre et de la demande exige maintenant que

$$\{P + p(t)\}\{A_0 + A_1 p(t - \theta)\} = C. \tag{12}$$

Pour l'équilibre à la longue on aura $C = A_0 P$; nous supposerons que cette condition est remplie, d'où:

$$PA_1 p(t - \theta) + A_0 p(t) + A_1 p(t) p(t - \theta) = 0. \tag{13}$$

La présente équation n'est plus linéaire. La solution $p(t) = Cz^t$ n'est plus possible. Posons maintenant

$$p(t) = \sum_1^\infty {}^k C_k z^{kt},$$

où C_k sont des constantes.

$$\text{On aura: } p(t - \theta) = \sum_1^\infty {}^k C_k z^{kt - k\theta} \text{ et}$$

$$PA_1 \sum_1^\infty {}^k C_k z^{kt - k\theta} + A_0 \sum_1^\infty {}^k C_k z^{kt} + A_1 \sum_1^\infty {}^k \sum_1^\infty {}^l C_k C_l z^{(k+l)t - k\theta} = 0. \tag{14}$$

Cette équation doit être une identité en t pour chaque valeur de t; il faudra que tout coefficient de z^{kt} disparaisse; pour $k = 1$ on a:

$$C_1(PA_1 z^{-\theta} + A_0) = 0.$$

De cette équation on déduit qu'il y a deux possibilités:

(1) C_1 est arbitraire $\neq 0$ et $z^{-\theta} = \dfrac{-A_0}{PA_1}$, (15)

ou

(2) $C_1 = 0$ et z est arbitraire. La signification de ce deuxième cas n'est qu'un remplacement de z par z^2; on peut donc y renoncer.

Grâce aux restrictions (7) nous ne considérerons que les cas où $A_0/PA_1=1$, et par conséquent

$$z^{-\theta}=-1. \quad (15')$$

Pour $k=2$, on a:

$$(PA_1z^{-2\theta}+A_0)C_2+A_1C_1^2z^{-\theta}=0,$$

ce qui devient à l'aide de (15'):

$$C_2=\frac{C_1^2}{2P}.$$

Pour $k=3$ on a l'équation

$$C_3PA_1z^{-3\theta}+C_3A_0+A_1C_1C_2(z^{-\theta}+z^{-2\theta})=0,$$

prenant la forme

$$0C_3+0=0,$$

C_3 sera donc arbitraire, etc.

De cette manière il est possible de déterminer les constantes C_k par des équations successives, tandis que la première équation de la série, l'équation (15) ou (15'), nous donne la valeur de z et, par conséquent, la période de $p(t)$. En comparant le résultat obtenu avec celui du §2, nous voyons qu'aussi maintenant les constantes C_1, C_3, C_5, etc. peuvent être choisies arbitrairement (c. à. d. en accordance avec le mouvement initial, entre le moment 0 et le moment θ par exemple). Tandis que dans le cas du §2 les constantes C_2, C_4, etc. doivent être égales à zéro, dans le cas présent les valeurs de ces constantes obéissent aux équations

$$C_2=\frac{C_1^2}{2P},$$

$$C_4=\frac{2C_1C_3-C_2^2}{2P},$$

etc.

La période de z et de $p(t)$ est la même dans les deux cas, à savoir $T=2\theta$.

Remarque. Il est possible de réduire ce problème[7] à celui du §2, par les transformations suivantes. L'équation (13) devient, tenu compte de (15'):

$$b^2p(t)p(t-\theta)+b[p(t)+p(t-\theta)]=0, \quad (16)$$

où $b=1/P$.

[7] Je dois à M. le Prof. J. Droste, professeur de mathématiques à l'Université de Leyde, les indications suivantes.

Cela se transforme en:

$$[bp(t) + 1][bp(t - \theta) + 1] = 1,$$

$$\log [bp(t) + 1] = - \log [bp(t - \theta) + 1].$$

En posant $\log [bp(t)+1]=\omega(t)$, on a:

$$\omega(t) = - \omega(t - \theta),$$

équation de la forme (2); la solution générale de cette équation sera:

$$\omega(t) = \sum_0^\infty{}^k C_{2k+1} z^{(2k+1)t},$$

d'où

$$p(t) = \frac{1}{b}[e^{\sum C_{2k+1} z^{(2k+1)t}} - 1].$$

Le développement en série de cette fonction en puissances de z^t mène aux coefficients trouvés ci-dessus.

5. Bien de consommation; "marché fermé"

Supposons maintenant que le pouvoir d'achat C n'est pas une constante, mais dépend de l'activité même de la branche considérée. Supposons de plus que le procédé de production demande à chaque moment de sa durée θ le même nombre d'ouvriers. En tenant compte du fait que les ouvriers travaillant au moment t préparent l'offre pour la période de t à $t+\theta$, le nombre occupé dans la branche (l'"activité") sera

$$N = \alpha \int_{t-\theta}^{t} d\tau [A_0 + A_1 p(\tau)]. \tag{17}$$

Le salaire par unité de temps étant S, le pouvoir d'achat sera SN et l'équation (12) se transforme en

$$[P + p(t)][A_0 + A_1 p(t - \theta)] = S\alpha \int_{t-\theta}^{t} d\tau [A_0 + A_1 p(\tau)].$$

Pour l'équilibre à la longue on aura $PA_0=\alpha S\theta A_0$, d'où $S\alpha=P/\theta$; il reste:

$$A_0 p(t) + A_1 p(t - \theta) p(t) + P A_1 p(t - \theta) = \frac{P A_1}{\theta} \int_{t-\theta}^{t} d\tau p(\tau) \cdots. \tag{18}$$

C'est de nouveau une équation fonctionnelle non-linéaire; nous posons de nouveau $p(t)=\sum_1^\infty{}^k C_k z^{kt}$. On a:

$$\int_{t-\theta}^{t} d\tau A_1 p(\tau) = A_1 \sum_1^{\infty k} \frac{C_k z^{kt}}{k \log z}(1 - z^{-k\theta});$$

l'équation (18) se transforme en:

$$A_0 \sum_1^{\infty k} C_k z^{kt} + A_1 \sum^{k} \sum^{l} C_k C_l z^{kt-k\theta+lt} + PA_1 \sum^{k} C_k z^{kt-k\theta}$$

$$= \frac{PA_1}{\theta} \sum^{k} \frac{C_k z^{kt}}{k \log z}(1 - z^{-k\theta}).$$

De cette équation on déduit, comme dans le §4, une série d'équations successives, dont la première nous donne la valeur de z, et les suivantes la valeur des constantes C_1, C_2, etc. Comme nous nous intéressons principalement à la période de z, nous ne discuterons que la première équation. Elle est, après division par C_1 qui paraît être arbitraire,

$$1 - z^{-\theta} = \left[\frac{A_0}{PA_1} + z^{-\theta}\right] \theta \log z. \tag{19}$$

Grâce aux restrictions (8) que nous nous imposons, nous ne considérons que les cas où $A_0/PA_1 = 1$ et l'on a donc:

$$\frac{1 - z^{-\theta}}{1 + z^{-\theta}} = \theta \log z.$$

Le premier membre se transforme, comme l'on voit facilement à l'aide d'une figure géometrique du plan complexe, en

$$i \operatorname{tg} \frac{\theta \arg z}{2}; \text{ on a donc:}$$

$$\operatorname{tg} \frac{\theta \arg z}{2} = \theta \arg z,$$

d'où suit, pour la solution qui nous intéresse:

$$\frac{\theta \arg z}{2} = 66 \text{ à } 67°,$$

et

$$T = 2.7\theta \text{ environ.} \tag{20}$$

Ce résultat signifie que la dépendence du pouvoir d'achat de l'activité de la branche elle-même, c'est à dire le fait que le marché est fermé, mène à une période prolongée des oscillations.

Remarque. La question reste si la solution posée $p = \sum C z^t$ est la solution générale.

6. Bien de consommation de longue durée

Jusqu' ici nous avons supposé que la durée de la consommation du bien considéré peut être négligée; dans le paragraphe présent cette restriction sera abandonnée: soit cette durée de η unités de temps.[8] Il sera utile dans ce cas, d'entendre par p le loyer (pour un navire p.e. le fret), et d'étudier le marché du louage du bien. (Le prix d'achat sera à peu près proportionnel au prix de louage).

Pour ce marché l'offre se constitue de la production de toute une période η; en admettant que cette production soit donnée par la même fonction que l'offre dans le cas (1), on aura pour l'offre à notre marché:

$$\eta A_0 + A_1 \int_{t-\theta-\eta}^{t-\theta} p(t)dt, \tag{21}$$

En prenant pour la demande la fonction

$$B_0 - B_1 p(t),$$

l'équation de l'équilibre au moment t sera:

$$A_1 \int_{t-\theta-\eta}^{t-\theta} p(t)dt = - B_1 p(t).$$

Nous posons de nouveau $p(t) = Cz^t$; on trouvera de la même manière que dans le §2 que C est arbitraire et que

$$\frac{A_1}{B_1}(z^{-\theta} - z^{-\theta-\eta}) + \log z = 0.$$

La restriction $|z| = 1$ demande que log z, et, par conséquent, $z^{-\theta} - z^{-\theta-\eta}$ soit purement imaginaire, d'où suit que

$$\arg z = \frac{2\pi}{2\theta + \eta},$$

ou

$$T = 2\theta + \eta. \tag{22}$$

On voit que pour $\eta \equiv 0$, la formule antérieure est retrouvée; pour $\theta << \eta$ on peut écrire $T = \eta$, indiquant que la durée de consommation est à peu près égale à la période des oscillations. Cette relation sert de base à la théorie du mouvement cyclique général de De Wolff.[9]

[8] Comme on le verra les considérations suivantes sont aussi valables pour les biens instrumentaux (moyens de production).

[9] Voir S. de Wolff, *Het economisch getij*, Amsterdam, 1929 (J. Emmering), p. 415–419.

7. Bien de consommation de longue durée; marché fermé

Maintenant il est facile de combiner les cas des §§5 et 6 en considérant le marché d'un bien de consommation de longue durée où le pouvoir d'achat des acheteurs jaillit de l'activité de la branche elle-même. Selon les principes exposés plus haut on aura:

$$\left[A_0\eta + \int_{t-\eta-\theta}^{t-\theta} A_1 p(\tau)d\tau\right][P + p(t)] = l\left[A_0\theta + \int_{t-\theta}^{t} A_1 p(\tau)d\tau.\right]$$

En indiquant la première intégrale par T_1, la deuxième par T_2, et A_0/A_1 par a, on a:

$$a\eta p(t) + p(t)T_1 + PT_1 = lT_2. \tag{23}$$

La manière la plus simple d'obtenir une solution me semble être maintenant de poser

$$\int^t p(\tau)d\tau = \sum_1^\infty {}^kC_k z^{kt}.$$

On en déduit:

$$p(t) = \sum_1^\infty k\log z \cdot C_k z^{kt},$$

$$T_1 = \sum_1^\infty {}^kC_k z^{kt-k\theta}(1 - z^{-k\eta}), \quad T_2 = \sum_1^\infty {}^kC_k z^{kt}(1 - z^{-k\theta}).$$

L'équation (23) devient:

$$a\eta \sum {}^k k \log z \cdot C_k \cdot z^{kt} + \sum {}^k \sum {}^l k \log z C_k C_l z^{kt+lt-l\theta}(1 - z^{-l\eta}) + \sum {}^k P C_k z^{kt-k\theta}(1 - z^{-k\eta}) = l \sum {}^k C_k z^{kt}(1 - z^{-k\theta}). \tag{24}$$

La première des équations suivant cette identité en z^t nous donne

$$a\eta \log z + Pz^{-\theta}(1 - z^{-\eta}) = l(1 - z^{-\theta}),$$

ou

$$\frac{a}{P}\log z + \frac{1}{\eta}(z^{-\theta} - z^{-\theta-\eta}) + \frac{1}{\theta}(z^{-\theta} - 1) = 0.$$

Le premier terme de cette équation n'ayant pas, selon nos restrictions (8), de composante réelle, il faut que la composante réelle du deuxième terme soit l'opposé de celle de la troisième. Cela posé il n'est pas difficile de trouver, pour un rapport quelconque de θ et η, le rapport entre T et θ. Ce rapport-là n'est pas tout à fait constant. Pour $\eta \leqq 10\theta$ on a par approximation:

$$T = 2.7\theta + 1.3\eta. \tag{25}$$

Pour $\eta \gg \theta$ on a $\lim_{\theta=0} \dfrac{T}{\eta} = 1.$

On voit de nouveau que, comparé au cas précédent, il y a un prolongement de la période à cause de la "fermeture" du marché. On voit de plus que pour $\eta = 0$, notre formule correspond au numéro (20).

8. Marchés liés

La méthode développée dans les paragraphes précédents peut être employée aussi dans des cas où l'on considère plusieurs marchés liés entre eux, comme les marchés des divers biens de consommation (liaison horizontale) ou les marchés d'un produit fini et des demi-fabricats et des matières premières, employés dans le procédé de sa production (liaison verticale). Dans cet article je me borne à un seul exemple de cet espèce, à savoir au cas d'une liaison verticale de deux marchandises I et II, I étant la matière première de II. On aura le schéma suivant:

Marché I: Prix $P+p(t)$; offre: $A_0+A_1p(t-\theta')$
demande: $B_0+B_1[q(t)-p(t)]$.
Marché II: Prix $Q+q(t)$; offre: $B_0+B_1[q(t-\theta'')-p(t-\theta'')]$
demande: $C_0-C_1q(t)$.

Comme unité de poids des deux marchandises on a pris des quantités correspondantes, de sorte que la production de l'unité de II demande une unité de I. Les frais de production ont été supposés constants. La forme de l'offre de II a été choisie telle qu'il n'y ait pas d'emmagasinage. Le délai entre le prix et l'offre est θ' pour marché I, θ'' pour le marché II.

On aura: $A_0 = B_0 = C_0$ et du reste:

$$\underbrace{A_1p(t-\theta'-\theta'') = B_1[q(t-\theta'')}_{\text{(équilibre au moment } t-\theta'')} \underbrace{- p(t-\theta'')]}_{\text{(équilibre au moment } t)} = -C_1q(t),$$

Posons $p(t) = k'z^t$ et $q(t) = k''z^t$; on aura:

$$A_1k'z^{-\theta'-\theta''} = B_1(k''-k')z^{-\theta''} = -C_1k''.$$

Les inconnues sont maintenant $k = k''/k'$ et z, des quantités complexes toutes les deux.

En éliminant k l'équation restante pour z sera:

$$\frac{1}{A_1} + \frac{z^{-\theta'}}{B_1} + \frac{z^{-\theta'-\theta''}}{C_1} = 0. \tag{26}$$

A_1, B_1, et C_1 étant des constantes positives, on verra sans peine à l'aide d'une figure géométrique du plan complexe que la possibilité de satisfaire à l'équation (26) n'existe que quand

$$\arg z \geqq \frac{\pi}{\theta' + \theta''};$$

il suit que

$$T \leqq 2(\theta' + \theta''), \tag{27}$$

où le signe inférieur n'est valable que quand $B_1 = \infty$.

Une réunion de ces deux marchés, de sorte que la production de la matière première et du produit fini sont exercées par les mêmes personnes, nous ramènerait au cas du §2, $\theta' + \theta''$ de (27) ayant la même signification que θ antérieurement. Il est remarquable que cette réunion signifie, selon (27), en même temps un ralentissement du mouvement oscillatoire.

9. Résumé des résultats

A. Période des oscillations.—

No.	Nombre des Marchés	Durée de la production	Durée de la consomm.	Spéculation	Pouvoir d'achat des consomm.	Période de l'oscillation	Remarques
1	1	θ	0	sans	const.	$T = 2\theta$	—
2	1	θ	0	avec	const.	$1{,}33\theta < T < 2\theta$	(1)
3	1	θ	0	sans	(2)	$T \backsimeq 2.7\theta$	(1)
4	1	θ	η	sans	const.	$T = 2\theta + \eta$	(1)
5	1	θ	η	sans	(2)	$T \backsimeq 2.7\theta + 1.3\eta$	(1)
6	2	θ', θ''	0	sans	const.	$T \leqq 2(\theta' + \theta'')$	—

(1) Seulement pour les oscillations strictement périodiques (pas amorties ou "explosives").
(2) Proportionnel à l'activité de la branche.

B. Forme des oscillations.—Le degré d'amortissement dépend en général des élasticités qui jouent un rôle dans le problème. La forme de l'oscillation pendant une période dépend d'un mouvement initial; le mécanisme de la propagation n'a qu'une influence indirecte: elle défend quelques formes spéciales, p.e. la forme sinusoidale, quand l'équation de la propagation n'est pas linéaire.

C. Les différences de phase entre le prix, l'activité et l'offre dans le "baromètre" du marché considéré.—Par une définition de l'"activité" comme celle donnée dans le §5 on rend possible la construction d'un baromètre indiquant le mouvement du prix et de l'activité dans leur succession caractéristique. Ces baromètres ont été construits dans la Fig. 2.

D. Remarque finale.—Il est évident qu'un nombre immense de problèmes a été négligé dans les considérations précédentes, et qu'un dé-

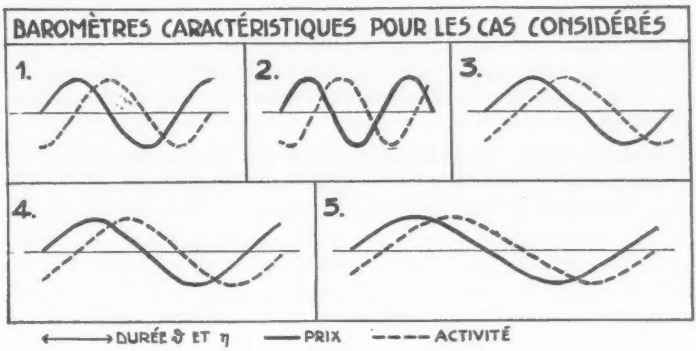

Fig.2.

veloppement en des sens bien divers est bien nécessaire avant que la discipline des oscillations économiques ait une base solide.

Scheveningen, Hollande

A CERTAIN ERRATIC TENDENCY IN ACCOUNTANTS' INCOME PROCEDURE

By JOHN B. CANNING

Paper read before the joint meeting of the Econometric Society and Section K of the American Association for the Advancement of Science, New Orleans, Louisiana, January 1, 1932.

I OFFER for consideration four propositions: (1) Sound work in the mathematical theory of price economics is unlikely to be done by one whose theory of income accounting is inchoate or otherwise defective.

(2) Generalizations drawn from statistical analyses in price economics by one who is unaware of the deficiencies in prevailing income accounting procedure cannot be adequately safeguarded by the usually calculated measures of probable error.

(3) The full fruition of quantitative economic theory must wait upon the substantial betterment of both the mathematical theory and the statistical practice of income accounting.

(4) Extensive rationalization of economic conduct in the economic arts is not to be expected unless, and until, the erratic character of prevailing income accounting is remedied.

Needless to say, I shall not attempt an exhaustive, formal proof of these four propositions: I shall be content to show the mathematical character of a portion of income accounting theory, to show the statistical unreliability of the related prevailing procedure, and to suggest the influence of this unreliable procedure upon economic events.

The statistical work of income accounting consists almost wholly of treating certain kinds of time series. The procedures employed lead to many kinds of erratic fluctuations in the terms of these series.

I shall speak of only one of these erratic tendencies; one that results in violent fluctuations in the successively reported annual net income[1]

[1] I use the term "annual net income" for verbal convenience. The figure meant is a figure determined by the formula: $(C-C_p)-(D-D_p)$ = net income of the year. In this expression C means "the sum of book credits to the 'proprietary accounts' during the year." C_p means "the sum of book credits to the 'proprietary accounts' during the year arising out of proprietary contributions," (e.g., shares of stock subscribed for), D means "the sum of all book debits to the 'proprietary accounts' during the year," and D_p means "the sum of all book debits to the 'proprietary accounts' during the year arising out of distributions of assets or promises to distribute assets to those beneficially interested in the proprietorship" (e.g., dividends declared). For convenience also I include within the term "proprietary accounts" the liability accounts arising out of borrowing transactions, but not the liability accounts arising out of goods and services purchased but not paid for.

figures of enterprises. Of course no professional accountant supposes that the figure he finds for any one year represents a fact exactly provable.[2] He regards the value found as a most probable measure of a tendency; and he regards the first differences of the successive annual figures as most probable changes in tendency. But one cannot suppose, on the other hand, that accountants perceive that their procedure *itself* is erratic, or, in other words, that any material part of the error in the successive annual figures is the inevitable consequence of their erratic algebraic treatment of their primary, factual data. The erratic tendency of which I shall speak is attributable to the mode of converting certain classes of discrete money outlays into equivalent expense deductions.

Aside from negligible contributions and payments in kind, the primary time series of income statistics consist solely of the incoming and the outgoing of money. All incomings except proprietary contributions and all outgoings except distributions to those having an interest in the proprietorship are included in these primary series. Incomings are positive, outgoings negative in sign. As a measure of income, the mere algebraic sum of receipts and payments during a year is useless except as an accounting for the change between beginning and ending cash balances. The accountant's problem is to convert the time series of actual outlays and actual receipts into equivalent series of incomes and expenses, that is, into positive and negative incomes. This conversion is, in essence, a matter of leading and lagging time series and of distributing the leads and lags.

In making this conversion there are two objectives; an implied objective that the algebraic sum of receipts and outlays shall, for the entire life of the enterprise, be equal to the algebraic sum of all annual net incomes for the like period; and an expressed objective that from the positive terms in each annual summation there shall be subtracted due allowances (expenses) for all outlays made (or expected to be made) *as an incident* to acquiring that year's positive, or gross, income. All accountants attain the implied end, none, the expressed end. The objectives are proper. The full usefulness of accounting cannot be enjoyed without reaching them.

The major processes involved in reaching the objectives may, as to effect, be described as follows: (1) Each discrete dollar-receipt and dollar-outlay is distributed as a continuous variable, "dollar-years of receiving" (income) and "dollar-years of paying" (expense); (2) the time limits within which each variable is to be continuous are fixed;

[2] I have shown in my *Economics of Accountancy*, pp. 94–99 and 126–127, that the figure for the net income of an enterprise becomes a provable fact only upon the termination of the enterprise.

(3) the equations, or formulas, of the variables are selected; (4) from these formulas the amount of each variable falling between the ordinates at the limits of the year in question is calculated; and (5) the algebraic sum of all concurrent terms is taken. This latter sum is the net income for the year.

Within the scope allotted to this paper I shall be unable to go into the details of the various expense-spreading formulas. I can do no more here than sketch a representative resultant of the commoner procedures. For convenience, I shall consider the case of a concern whose sole important source of money receipts is collections from customers. For this concern I shall separately carry the major classes of annual income terms through the first four of the major processes referred to above.

The treatment of the positive terms is simple. Since, in our assumed example, receipts of money are restricted to customer collections, the variable, "sales less bad debt losses," may be substituted for collections. A dollar collected from a customer will be represented by a dollar of income in the year in which the sale occurred, without regard to whether the collection was made in the year prior to the sale, or during that year, or in the year following the sale. The summation of sales made during the year, adjusted for bad debt losses, becomes the sum of the year's positive terms. Annual gross income figures are therefore sensitively responsive to changes in trade conditions.

The negative terms I shall divide into two major classes, one of which will be sub-divided. One of these major classes arises out of outlays made in hand-to-mouth buying; the other, out of outlays made to procure long-lived facilities and in connection with long-term commitments.

The cycle, or turnover period, for the first class is short. Wage payments to factory workmen, to salesmen, and office staffs, payments for materials and supplies utilized in operations, hired services like transportation, etc., are representative of the class. These payments are converted into expenses of the period in which the goods resulting from the hired services and the utilization of the materials are sold.[3] Again,

[3] Superficially, these payments seem to become expenses of the year in which the materials were bought and in which the services were rendered. Purchases are charged in the income accounts as they are made, wages, as they are earned. But at the end of the year partly manufactured goods and finished, but unsold, goods will be inventoried. The figure found by the accountant as the "cost" of the inventory will become a credit to income, that is, will be minus a negative term. This inventory figure will become a negative term in the next year's summation. A like carry-over, by means of the inventory valuation, occurs for certain variables of the other major class of negative terms also. Clerically regarded, there is a primary distribution and an adjustment, or redistribution, of the leads and lags.

the date of the actual outlay is immaterial; it may precede the year in which the expense is deducted, fall in the same year, or follow in a later period. But characteristically, the interval between the time of *payment* and the time by which an expense to the full amount of the payment has become *charged*, is short. The annual summations of this class of expense, like those of the positive summations, are, therefore, sensitively responsive to changes in trade conditions. The absolute values of the successive annual summations of this class of negative terms are highly correlated positively with the concurrent positive summations.

The second major class of negative terms is usually represented as three or more coordinate, independent classes. Superficially, they appear quite unlike one another; economically and statistically, their kinship is near. With respect to the theoretical problem of income determination, they are essentially alike.

One of these sub-classes consists of the so-called "non-disbursed" expenses, depreciation and obsolescence. In reality they arise out of numerically equivalent outlays just as all other expenses do; in general, the expense terms merely continue long after the outlay has been made. The original outlay made to acquire a long-lived instrument, a leasehold, etc., gives rise to a series of expense terms that is continuous from the time the thing acquired goes into use until the last product associated with its use is sold.[4] The formula most used in distributing this expense is the "straight line" formula. That is, the annual negative terms are equal.[5] The formula next in popularity substitutes, for this arithmetic progression of common difference zero, a geometric progression with common ratio of 1 plus a small constant that is usually called a rate of interest, although it is not a rate of interest. The deduction terms according to this formula are inert and unresponsive to trade conditions.

A second sub-class of expense terms arises out of outlays that, though repetitive at short intervals, are associated with long term commitments. Monthly rental instalments on long term leases, salaries of officers, etc., suggest the type. In accounting practice, the annual expense terms are nearly equal, after end-of-the-year adjustments, to the yearly outlay. These expense terms also are inert and insensitive to trade changes.

The third sub-class I shall call, for short, service charges. These ex-

[4] For the sake of simplicity I have neglected the treatment of expected salvage value, a positive term. In practice, the difference between cost and salvage value is distributed—a procedure which, in effect, distributes the items simultaneously and in like proportions.

[5] But see note (3) above, concerning redistributions.

penses arise out of outlays incurred for repairs and upkeep, for general property taxes, for insurance premiums, and the like. The amount deductible annually is initially equal to the liabilities for payment incurred during the year; some of these deductions are adjusted in the inventory account. The annual summation of this class is, to a slight degree only, responsive to trade conditions. For example, machine repairs fluctuate with amount of work done, but such outlays are also a function of the age-distribution of the machines. There is often a spurious appearance of dependence; large programs of repair are often deferred until increasing operations supply the ready funds to pay for them. This is notoriously the case with many American railroads. As a whole this sub-class fluctuates little from year to year and the absolute correlation with the concurrent positive terms is small.

Considered as one major class, these three sub-classes of negative terms associated with long-lived facilities and long-term commitments, exhibit in the annual summations large absolute amounts and a very small coefficient of variation.

Let us take stock of our three major series, one positive, the other two negative. The positive terms fluctuate in response to trade conditions. One class of the negatives is highly correlated absolutely with the concurrent terms of the positive series. Hence the annual differences of the first two exhibit a coefficient of variation not greatly differing from that of either the minuend or the subtrahend series. But when we deduct from these fluctuating difference terms, the concurrent terms of a large, nearly constant series, we reach a final series of net annual income figures whose relative fluctuations are so violent as to suggest the term "freakish," rather than the milder term, "erratic" behavior.

This violent fluctuation is not a new phenomenon to those who have had to deal with income statistics in income studies, in business cycle analyses, in income taxation problems, and the like. It is possible, however, that many may not hitherto have marked the degree to which mere accounting procedure, rather than real economic events, has brought about this behavior of income series.

I propose certain questions for consideration: Does the amplitude of fluctuation in annual net income series arise chiefly by reason of events that have a real existence rather than by reason of the arbitrary mode of accounting for the events? Is there, in economic reality, any such fluctuation in real net earnings as is exhibited in the accountants' *figures* for earnings? Is there, in economic fact, a large class of fixed expenses, that is, of annual expenses that are independent of the annual volume of operations? I shall answer each of those questions in the negative. The large net income figures found by the accountants in

years of large volume of output are characteristically grossly overstated; the red ink that appears in the income balances of low-volume years,—because of the arbitrary treatment of "fixed" expenses—is, more often than not, just ink. There is no kind of expense that is, unconditionally, and in economic reality, a fixed expense with respect to annual operating income; there are only arbitrarily fixed *allocations* of certain expenses.

Before I attempt to support my negative answers let me recall the accountants' expressed objective: to deduct from each year's gross income the expenses incurred in *getting* that gross income. The gross income in question should be not money nor collections, but should be a measure of the *coming-in* of money (at some time or other) by reason of operations *brought to a certain stage* (sales), *during the specific year*. The expense in question should not be money or payments, but should be a measure of the *going-out* of money (at some time, past or future) made necessary by these same operations that have been *brought to this stage during the year*.

The accomplishment of this highly desirable objective amounts to solving the problem of how much of any given dollar of outlay is spent to obtain any particular dollar of receipt. I shall not attempt to answer this question specifically myself; I shall show that the accountants' answers are needlessly wide of the mark because of their treatment of the second major class of negative terms. I shall also indicate the mathematical character of this portion of income accounting theory.

This class includes some of the so-called fixed charges and all of the so-called fixed expenses. In certain senses they are really fixed. If $1,000 is paid for a machine the outlay is fixed in the sense that it is an unalterable number of dollars of negative sign. It creates a fixed *total* of expense, that is, of charges against income. That expense total is a fixed expense of the total time within which the machine's use enhances sales. But the expense is not fixed in the sense that any particular portion of it is unconditionally a negative term in the income summation to be charged to any particular year within the time limits of use. The latter is a matter of distribution and is entirely at the disposal of the accountant. The accountant can have no option as to the total ultimate expense charge; he cannot control the time within which all of a set of services will have been utilized, but he can distribute the fixed total in the actual time of use in any manner that recommends itself to his judgment.

Exactly the same propositions can be made about each and every one of the series in my second major class of negative terms. The contracting to pay $10,000 yearly rental for 50 years for a parcel of land is not evidence of a judgment that the land's services will be of ex-

actly equal effect in enhancing sales in each year. It is not evidence of any judgment at all except that the use of the land for *fifty* years will be worth as much, in producing revenues, as the capital value of an annuity of $10,000 a year for 50 years. Beyond this judgment the bargain merely indicates that the parties prefer the annuity method of settlement. Again, $500,000, the outlay, is the fixed distribuendum, 50 years is the fixed period, but the distribution is again at the disposal of the accountant.

If during the life of a machine it will pay to spend $500 on repairs, and if $500 is thus spent, the $500 must be distributed during the use-life of the machine, but it can be distributed at will within that time.

Consider the conditions that give rise to the primary outlays of this class. Concurrent selling prices and quantities sold are, from a static viewpoint, negatively correlated. But we all know, that for brief periods,—say during a business cycle—correlations may become positive and large. It is this latter, short run condition that makes economical the provision of plant capacity in excess of that most economical for the expected minimum rate of production. And it is this decision to create, to maintain, and to operate (when advantageous), a plant of some particular capacity that fixes the aggregate of these so-called fixed expenses. The totals are capacity functions; only indirectly are they rate of operation functions.

If it is true, as I believe, that capacity in excess of requirements for minimum rates of operation can legitimately be provided only in order to take advantage of favorable markets, then the test of whether or not the provision of a given excess capacity has been profitable may be stated thus: Suppose the quantities produced in unit time to have been graduated by increments between minimum expected rates and maximum possible rates. Under this supposition, have the sales receipts from operation at maximum capacity been great enough to absorb (1) all deductions *per dollar of sales* appropriate to production at the next lower capacity increment plus (2) the entire differential outlay necessary to providing this uppermost capacity increment together with interest on the outlay for this differential capacity at a rate acceptable to the management? To be sure, the question, "has the excess capacity actually provided been profitable," sounds academic, but an answer to it is indispensable to answering the further vital question, will it probably pay to increase an existing capacity? For that is a continuing question that must be answered if an enterprise is to attempt to maximize the present worth of its gains. The answers to the questions just asked come within the scope of the accountants' expressed objective in income and expense distribution. If their procedure is to supply data for answers, customary methods of distribution must be altered.

The form of the questions just put suggests the character of the contemplated changes in procedure. It suggests the finding of a standard, or base charge, a negative magnitude k that is to be modified yearly[6] in accordance with some index of quantities produced and some index of selling prices. The properties desirable in the formula may be described as follows. The standard charge, k, should be a magnitude such that: (1) were a plant built of capacity best suited to constant quantity production at an expected mean rate (as measured by an index number of quantities) and were the plant operated constantly at that rate, $\sum k$ during the life of the plant, would equal the sum of the outlays incurred[7] in obtaining the services of the fixed facilities; and (2) were all sales to be made at some mean of expected selling prices (as measured by some index number of selling prices) the sales receipts would imply a return on the investment at the minimum rate acceptable to the management.

Then, for the plant actually built, k would become the charge against income in those years in which both the price index and the quantity index were equal to the values employed in determining k. In years in which either the price index or the quantity index was different the charge should become $k[1+f(p, q)]$ in which $f(p, q)$ is a function of selling prices and of quantities sold having the following properties: (1) it will be zero when the index of prices p, and the index of quantities q are both equal to the index values used in solving for k (as above); (2) it will equal -1 in periods during which operation is barely preferable to non-operation; (3) it will be less than -1 if operations will not pay;[8] (4) it will reach some value h in the least favorable market in which it would pay best to operate at full normal capacity;[9] and

[6] By "year" I shall mean "unit time."

[7] Both these sums are, of course, negative.

[8] This special case is interesting. It means this: if for an interval of time it will not pay to operate a plant, but, after the interval, it will pay to resume operations, the plant has a value, upon resuming operations, greater than its value when operations were interrupted. This suggests another property of the formula, viz.,

$$k[1+f(p, q)] \gtrless -siV_0$$

in which V_0 means "the sum of outlays (and of commitments) incurred for fixed facilities that, at the beginning of the idle time, had not been charged against income," i is "interest on 1 for unit time at the rate used in computing k," and s is the "shut-down time."

[9] If the value rises above h the condition of plant "overload" occurs. The meaning of values in excess of h is also interesting economically. It is the temporary condition under which it will pay best to operate at unit costs in excess of minimum possible unit costs, e.g., to avoid loss of a sale contracted at a high price, it may pay, for a short time, so to overload an electric generator that the resulting overheating greatly curtails its total ultimate economical output of energy.

(5) it can be so graduated that if future operations are as favorable as expected, (a) $\sum_{t=0}^{t=n} k[1+f(p, q)]$ will equal total outlays for fixed facility services during the enterprise life; and (b) the enterprise will have earned on its total investment the rate used in determining k[10].

I have been unable, as yet, to write $f(p, q)$ in terms of a price index and a quantity index in a form that is satisfactory to me, but I have made some approximations toward it. I think, however, that I have assigned properties to the function that are in accordance with the economic verities of the accountants' problem. I think also that a practically workable formula can be devised that would approximate the results of the theoretical function.

If my analysis is sound, it is clear that these so-called fixed expenses are, in economic fact, the most widely varying of all. Charges for the services of fixed facilities should fluctuate widely from year to year if trade conditions change instead of being nearly constant as accountants now distribute them. Per unit of product, or per dollar of sales, they should rise in good markets and fall in bad; the accountants' unit figures reverse the true direction of change.

Let me sum up the existing state of affairs. Superficially, the accountants' net income figures show huge net incomes in years in which plants operate to full capacity[11] and sales are made at high prices (or, what comes to the same thing, sales price minus selling expense per dollar of sales is high). In years in which plants operate at a small fraction of capacity the accountants show alarming deficits. But suppose

[10] Were such a function as $k[1+f(p, q)]$ to be employed it would be readily possible to compute a magnitude K representing the mean annual expected outlay for fixed facility services for the plant actually built. The difference,

$$\sum_{t=0}^{t=a} K - \sum_{t=0}^{t=a} k[1 + f(p, q)]$$

from the time of beginning operations to time a, would measure the accumulated margin of arrearage (if negative) from average expectations or margin of advantage experienced thus far (if the difference is positive) over original average expectations. It should be noted that both K and k are negatives. Note that K, thus defined, would be nearly equal to the accountants' "fixed expense."

[11] It should be pointed out that the net incomes found in such years could not be sustained indefinitely even if the concern could continue to operate at full capacity in an unchanged market. If the "straight line" method of charging depreciation on machinery and other facilities for which total service life is a function of the rate of exploitation is employed and if, in estimating the number of years a machine may be expected to last, allowance is made (as it usually is) for some idle time, then capacity operation will exhaust the economical life of the machine in a number of years less than the estimated number. This would necessitate, at some future time, a deduction for "depreciation arrearage." This is not the only reason, of course, why the accountants' net income figure could not be sustained under the assumed conditions.

we put the significant question, has the excess of sales receipts in the fat years of capacity production at high selling prices over the sales receipts in the lean years been large enough to pay for *all the costs of providing capacity in excess of minimum requirements* and still show a profit? Or we may put the question, were the sales receipts in the lean years smaller than the most probable expenses of getting them in an establishment for which the physical volume for that year would be an optimum? Were the incomes fat and lean or only the income figures? Toward the answering of these questions the accountants' annual figures give us exactly no help at all.

Now this periodic reversal of the microscope through which the accountants view income, would be bad enough if what they report had no repercussion upon the actual operating policy of enterprises; we should have paid for inferior statistics. But if, and to the extent that, business men *believe* what the accountants' figures seem to say, we have a pernicious state of affairs that probably to a large extent is responsible for the violence of business fluctuations in modern times.

Although I have spoken throughout my paper about certain erratic tendencies in the accountants' income data I speak of these tendencies as purely objective facts; I am not scolding the accountants. I am mindful that they might ask, what have economists and statisticians done in income statistics? What, indeed?

I have discussed in this paper only one of many erratic tendencies in income figures. A real understanding of all of them and an appreciation of the rôle they play in economic affairs is indispensable to the quantitative economic theorist. Erratic accounting does not generate business cycles, but, given an external generating impetus, belief in the accountants' figures leads to action that must increase the amplitude of the business cycle swings. The student of business cycles must miss his mark by a wide margin unless he not only knows accounting practice but also correctly values its influence in affairs. Erratic accounting does not give rise to credit inflations, but reliance by bankers on erratic accounts must often do so. The students of banking, of currency management, and of taxation, as well as the student of business cycles, have need of a like intimate knowledge of accounts.

I suggest that the betterment of income procedure is a proper task for either mathematical economists or for statistical economists. I suggest that if they attempt this task they will find it to be one that will tax their intellectual powers. I suggest also that the invention of substantial betterments that are really applicable in accounting practice will do as much to stabilize and rationalize economic behavior as anything else which economists are likely to contribute in the near future.

I venture to say in closing that economic science must defer its ma-

turity as a realistic, quantitative science that can claim a substantial degree of reliability in its analyses and predictions, until the statistical treatment of annual net income data is very greatly improved. I venture also to say that economists who continue to employ such income data as accountants now furnish, without being aware of their erratic character and without regard for the rôle that these same data play in influencing enterprise will simply repeat the fable of the blind men and the elephant.

Stanford University, California

THE ABSORPTION OF BANK CREDIT

By James Harvey Rogers

Paper read before the joint meeting of the Econometric Society and the American Economic Association, Washington, December 28, 1931.

In the present severe depression have been thrown into the lime-light as perhaps never before the phenomena of credit expansion and contraction. It is the purpose of this paper to analyze certain aspects of these processes with particular reference to the American banking system. In this way, among other conclusions, will be given at least a partial answer to the important question of why the central bank rediscount rate, usually so effective in credit expansion seems to possess but little potency in contraction.

An individual bank in the United States expands its credit primarily on the basis of its reserve balances. If these are increased above the amounts required by law and by the dictates of good business judgment, the excess is apt soon to be either loaned or invested. Any deficit in such reserve holdings on the other hand, while usually made up immediately by borrowing from one of the Federal reserve banks, is apt soon to lead to a contraction of loans and investments. Not only is continued large indebtedness by a member bank to one of the Federal reserve banks frowned upon by the central banking authorities, but it is apt also to be regarded by the bank's depositors as an element of weakness. Hence, in a period like the present one, the pressure toward contraction is very great.

Other than by credit contraction, a bank accumulates cash and reserve balances in three major ways: (1) Through contributions of stockholders or accumulation of profits; (2) Through advances from a Federal reserve bank or from a correspondent, or through the purchase of "Federal Funds;" (3) Through the receipt of deposits.

The first of these methods is of course the chief resort at the time of the organization of the bank. Later, earnings are accumulated and stock-holder assessments are far from rare.

After the bank has become a full-fledged operating unit, however, the other two means are apt to provide the only practical alternative to credit contraction. In periods of distress, not only do profits disappear but they are all too often turned into losses, producing a drain instead of an accumulation. Moreover, the effects of these losses are often accentuated by the natural inclination to maintain dividends although unearned. The assessment of stock-holders, on the other hand, is an emergency device seldom applied until the distress has

become very acute. The two more normal means of accumulation, therefore, should be considered in more detail.

In general it may be said that a bank is prosperous to the extent that it receives deposits, and unprofitable to the extent that it loses them. Every accretion to this life-blood of the bank's activities is good business, and every reduction is the worst of bad business. To the energetic banker, nothing is more gratifying and more reassuring than the accumulation of deposits, and nothing more depressing than their disappearance.

In contrast, an advance from a Federal reserve bank or from a city correspondent is regarded at best as but a temporary resort. To begin with, it is a debt contracted because the accumulation of deposits (the bank's main business) is at least temporarily insufficient to meet other obligations. To maintain such a debt for any considerable period of time is an admission of an over-extended credit position. Moreover, the Federal reserve banks, as well as city correspondents, insist that such advances be of the most liquid variety and of short maturity. Renewals, especially if often repeated, are frowned upon. It is easy to see, therefore, how reluctantly a bank in a weakened condition would rely upon such a device.

When the banking system as a whole is regarded, instead of the individual banks, the sources of new deposits are seen to be several: (1) Cash inflows from circulation; (2) Gold imports; (3) Increases in Treasury Currency outstanding; (4) Expansion of Federal reserve credit.

The ensuing analysis of these sources and of the action of the individual banks will amplify that of Prof. C. A. Phillips as given in his extremely stimulating little study *Bank Credit*, published more than a decade ago.[1] The analysis will be divided into A, B; I, II. A signifies that no cash is flowing into or out of circulation. B signifies that cash is flowing in "normal" quantities to circulation in case of credit expansion and in "normal" quantities from circulation in case of credit contraction.

(By "normal" quantities is meant such quantities as are required to keep M/M' constant, where M = average quantity of money in circulation for the period considered, and M' = average quantity of bank deposits subject to check for the period considered.)

I signifies that the new reserve coming to Bank A has been received in the form of deposits.

II signifies that the new reserve is received in some other way than as deposits.

[1] C. A. Phillips, *Bank Credit*, New York: The MacMillan Company, 1919.

Bank Credit Expansion

Case A_I.

Bank A receives as a deposit \$1 of new reserve. If no cash is flowing to circulation, the loan[2] *expansion x_1 of Bank A is computed as follows:*

Let r = reserve ratio for Bank A

Let d = derivative deposit[3]—loan ratio for Bank A

Let c = the overflow cash from Bank A to other banks as a result of the loan expansion x_1

Then

$$c = 1 - r - rdx_1$$

Also

$$c = (1 - d)x_1$$

Hence

$$(1 - d)x_1 = 1 - r - rdx_1$$

$$(1 - d + rd)x_1 = 1 - r$$

$$x_1 = \frac{1 - r}{1 - d + rd}$$

If $r = 10\%$ and $d = 20\%$

$$x_1 = \$1.097.$$

In other words, the loan expansion of Bank A is approximately 1.1 times the new reserve cash received as a deposit.

Case A_{II}.

Bank A receives \$1 new reserve in some other way than as a deposit (as an advance from the central bank for example). If no cash flows to circulation the loan expansion of Bank A is computed as follows:

$$c = 1 - rdx_1$$

$$c = (1 - d)x_1.$$

Hence,

$$(1 - d)x_1 = 1 - rdx_1$$

$$(1 - d + rd)x_1 = 1$$

$$x_1 = \frac{1}{1 - d + rd}.$$

If $r = 10\%$ and $d = 20\%$, the loan expansion for Bank A is approximately 1.22 times the new cash received.

[2] Loan expansion x_1 is used to designate loan and investment expansion.

[3] By a derivative deposit is meant one derived from a loan or investment of the bank, or built up to meet the loan maturity. d varies from bank to bank but in the United States probably averages 10–20 per cent.

The deposit expansion of Bank A in each of these two cases is as follows:

$$\text{A}_{\text{I}} \qquad x_d = 1 + \frac{(1-r)d}{1-d+rd} = \frac{1}{1-d+rd}$$

which is the same as the loan expansion in Case A_{II}

$$\text{A}_{\text{II}} \qquad x_d = \frac{d}{1-d+rd}.$$

It will be noted that the above computations of x_1 and x_d apply only after the assumed adjustments have taken place.

For the banking system as a whole the loan and deposit expansions are made up of the expansions for the individual banks of the system. When Bank A expands its loans, it loses in overflow cash $(1-d)$ times this expansion. The cash lost, however, goes to other banks as deposits and with them becomes the basis of further loan expansions. These in turn produce cash outflows to still other banks, and they in their turn continue the expansion.

These expansions can be summed as follows:

Case A_I

$$\begin{aligned}
X_1 &= \frac{1-r}{1-d+rd} + \frac{(1-d)(1-r)}{1-d+rd}\cdot\frac{1-r}{1-d+rd} \\
&\quad + \frac{(1-d)^2(1-r)^2}{(1-d+rd)^2}\cdot\frac{1-r}{1-d+rd} + \cdots \\
&\quad + \frac{(1-d)^{n-1}(1-r)^{n-1}}{(1-d+rd)^{n-1}}\cdot\frac{(1-r)}{1-d+rd} + \cdots \\
&= \frac{1-r}{1-d+rd}\left(1 - \frac{(1-d)(1-r)}{1-d+rd} + \frac{(1-d)^2(1-r)^2}{(1-d+rd)^2}\right. \\
&\quad \left. + \cdots + \frac{(1-d)^{n-1}(1-r)^{n-1}}{(1-d+rd)^{n-1}} + \cdots\right) \\
&= \frac{1-r}{1-d+rd}\cdot\frac{1-d+rd}{r}, \qquad \left(\text{since } \frac{1-r}{1-d+rd} < 1\right) \\
&= \frac{1-r}{r}.
\end{aligned}$$

Case A_{II}

$$X_1 = \frac{1}{1-d+rd} + \frac{(1-d)}{1-d+rd}\cdot\frac{1-r}{1-d+rd} + \frac{(1-d)^2(1-r)^2}{(1-d+rd)^2}\cdot\frac{1}{1-d+rd} + \cdots + \frac{(1-d)^{n-1}(1-r)^{n-1}}{(1-d+rd)^{n-1}}\cdot\frac{1}{1-d+rd}\cdot$$

$$= \frac{1}{r}\cdot$$

It will be noted that in each case the result is independent of d, and when $r = 10\%$, the loan expansion for the banking system is 9-fold the new reserve cash for Case A_I and 10-fold for Case A_{II}.

The deposit expansions similarly arrived at are as follows:

A_I $$X_d = \frac{1-r}{r} + 1 = \frac{1}{r}$$

A_{II} $$X_d = \frac{1}{r}\cdot$$

If, with the general expansion of loans and deposits, cash flows in normal quantities to circulation, we have Case B.

$$\text{Let} \qquad \frac{M}{M'} = k.$$

Let r = average reserve ratio for the banking system. Then:[4]

B_I $$rX_d + kX_d = \$1$$

$$X_d = \frac{1}{r+k}$$

$$X_1 = \frac{1}{r+k} + k\left(\frac{1}{r+k} - \frac{1}{1+k}\right)$$

B_{II} $$rX_d + kX_d = \$1$$

$$X_d = \frac{1}{r+k}$$

$$X_1 = \frac{1}{r+k} + k\left(\frac{1}{r+k}\right)$$

[4] For bank to bank expansion, see the author's *Stock Speculation and the Money Market*, Yale University Press, 1927.

If $r=10\%$ and $k=16\frac{2}{3}\%$ as is approximately true in the United States, X_d for both Case B_I and Case B_{II} becomes \$3.75 as opposed to \$10 for cases A_1 and A_2. Thus the normal outflow of cash to circulation reduces by almost two-thirds the deposit expansion of the banking system. The reduction in loans and investments is but slightly less.[5]

Bank Credit Contraction

For bank credit contraction, the formulae are entirely similar to the ones given above.

A_I: The reserve is lost through deposit withdrawal and no cash is returned from circulation as a result of the loan and deposit shrinkage.

$$s_1 = \frac{1-r}{1-d+rd}$$

$$s_d = \frac{1}{1-d+rd}$$

$$S_1 = \frac{1-r}{r}$$

$$S_d = \frac{1}{r}.$$

A_{II}: The reserve is lost in some other way than through deposit contraction and no cash is returned from circulation as a result of the loan and deposit shrinkage.

$$s_1 = \frac{1}{1-d+rd}$$

$$s_d = \frac{d}{1-d+rd}$$

$$S_1 = \frac{1}{r}$$

$$S_d = \frac{1}{r}.$$

B_I: The reserve is lost through deposit withdrawal and because of the resulting loan and deposit contraction cash is returned from circulation in quantities sufficient to maintain M/M' constant.

[5] Case B_I: $X_1=\$4.23$
Case B_{II}: $X_1=\$4.375$

$$S_d = \frac{1}{r+k}$$

$$S_1 = \frac{1}{r+k} + k\left(\frac{1}{r+k} - \frac{1}{1+k}\right).$$

B_{II}: The reserve is lost in some other way than through deposit withdrawal and because of the resulting loan and deposit contraction cash is returned from circulation in quantities sufficient to maintain M/M' constant.

$$S_d = \frac{1}{r+k}$$

$$S_1 = \frac{1}{r+k} + k\frac{1}{r+k}.$$

From the expansions above given, the position of the individual banks in periods of general credit increase is clearly seen. Once the initial impetus appears through the receipt by any one of the banks of new funds either as a deposit (Case I) or as an advance from one of the central banks or in any other way than as a deposit (Case II), the rest of the expansion is usually semi-automatic. The loan, investment, and deposit expansions of the receiving bank are followed first by an overflow of cash from that bank; second, by the receipt as deposits by other banks of this overflow cash; third, by the credit expansions in turn of these receiving banks; and so on ad infinitum.

Gradually, as the new reserves and deposits become more and more widely disseminated, buoyancy and enthusiasm spread throughout the system. The usual reluctance toward borrowing from the central banking institutions rapidly vanishes as active business, giving rise to sound discountable paper, in turn dissipates the reluctance of the central banks toward further lending.

Any individual bank which in such a period of credit expansion decides against increasing its loans and investments, will soon become a veritable sink-hole for the rapidly accumulating new reserves received from its sister institutions. So high indeed would cash and reserve balances soon become, that no living banking board—in its usual ignorance of the source of the funds it handles—would long continue such extravagant inaction.

Nevertheless, in such circumstances, central bank rate advances and other restrictive credit measures are peculiarly effective, except, of course, in cases of continued gold inflow. Since, in the United States at least, the whole expansion process—after the original impetus—is apt to be carried forward by continually increasing central bank advances,

all that is required to arrest it is effective action to curb, and to reduce, the existing volume of outstanding central-bank credit. Hence open-market sales combined with rate increases, or simple refusal to grant advances sought, are almost sure to accomplish the desired end.

In periods of general credit contraction, almost exactly the reverse situation holds. Once started, this process likewise feeds upon itself. Any bank, in order to meet a gold shipment or an outflow of cash to circulation, to liquidate an advance from one of the Federal reserve banks, or to counteract any other draft upon its reserves, is apt to curtail its loans or its investments. In doing so, cash is automatically drawn from other banks, which lose deposits to the extent of the withdrawals. On these banks is thereby imposed the necessity of further loan and investment contraction to restore their impaired reserve ratios. This contraction, in turn, depletes the reserves of still other banks, and thus the process once started continues until an inflow of funds again reverses the movement.

In such a situation, lowering the rediscount rate is of little effectiveness in arresting the often accelerated liquidation. At a time when the search for liquidity becomes little less than a mad scramble, the possibility of increasing debt at a Federal reserve bank *slightly more cheaply* provides no individual incentive to end the movement. Under the capitalist system, therefore, other types of relief are required.

Should a large gold inflow appear, or should currency return in quantities from circulation, a powerful, corrective, expansionary force would be felt. The new funds coming to the banks would counteract speedily and automatically the current disastrous drains, and remove therefore the chief driving force toward further bank credit liquidation.

In the absence of such beneficient inflows, over which no one seems to have any predictable control, a remedy completely at the disposal of our central banking authorities—once the gold scare can by the proper legislation be removed[6]—is apparently indicated. By Federal reserve "open-market purchases of bills and government securities," new funds—in every way just as effective as those derived from gold or currency inflows—can be turned over to our hard-pressed banks.

Under present distressing conditions, *easy money* is of little importance. *Plentiful money* would probably bring effective relief.

Yale University

[6] See paper entitled "Gold, Foreign Credits and Depression," read by the writer before the meetings of the American Statistical Association in Washington, December, 1931. Published in September, 1932 number of the Journal of the American Statistical Association.

THE ORGANIZATION OF THE ECONOMETRIC SOCIETY IN CLEVELAND, OHIO, DECEMBER, 1930

On Monday evening, December 29, 1930, a group of economists, statisticians, and mathematicians, whose names appear below, met in Cleveland, Ohio, U.S.A., at the invitation of Professors Irving Fisher, Ragnar Frisch, and Charles F. Roos, to form a new society, to be called The Econometric Society, an International Society for the Advancement of Economic Theory in its Relation to Statistics and Mathematics. At 8:00 P.M. the meeting was called to order. Professor Joseph Schumpeter, of Bonn, was elected chairman of the organization meeting and Charles F. Roos was elected recorder.

Professor Frisch circulated copies of a tentative constitution and explained that the draft was based on ideas which had been obtained in an extensive correspondence carried on by himself, Professor Fisher, and Professor Roos, with economists and statisticians throughout the world. Under the guidance of the chairman this constitution was revised and adopted. All of those present took an active part in the discussion and revision of the constitution. The chairman was directed to appoint a committee to phrase the constitution properly. F. C. Mills, Ragnar Frisch, and Charles F. Roos were appointed to this committee.[1]

When the constitution had been voted upon, Professor Schumpeter's motion that the Society should be considered founded was accepted unanimously. This was at ten o'clock.

The chairman proposed to proceed with the election of the first president and the Council of the Society, in accordance with the provisions of the constitution. The chairman named Professor Irving Fisher, of Yale University, in a short nominating speech, and Professor Fisher was unanimously elected the first president of the Econometric Society. After the election of the President, the following were elected to the Council:

Luigi Amoroso, University of Rome
L. v. Bortkiewicz, University of Berlin (Died August, 1931)
A. L. Bowley, London School of Economics
François Divisia, École Nationale des Ponts et Chaussées, Paris
Ragnar Frisch, University of Oslo
Charles F. Roos, Smithsonian Institution Building, Washington
Joseph Schumpeter, University of Bonn

[1] The Constitution as finally formulated appears in this issue of Econometrica.

E. B. Wilson, Harvard University
Wl. Zawadzki, University of Wilno

Professor Schumpeter was directed to notify Professor Fisher of his election and ask him to proceed with the organization of the Council and the election of a secretary and treasurer of the Econometric Society.

At 10:45 P.M. the first meeting of the Econometric Society adjourned.

ORGANIZING GROUP

Ragnar Frisch	M. C. Rorty
Harold Hotelling	Joseph Schumpeter
Karl Menger	Henry Schultz
Frederick C. Mills	W. A. Shewhart
William F. Ogburn	Carl Snyder
Oystein Ore	Ingvar Wedervang
J. Harvey Rogers	Norbert Wiener
C. F. Roos	Edwin B. Wilson

Correspondence may, for the present, be addressed to: Professor Irving Fisher, 460 Prospect Street, New Haven, Connecticut.

Respectfully submitted,

CHARLES F. ROOS,
Recorder

LA RÉUNION DE LA SOCIÉTE D'ECONOMÉTRIE, LAUSANNE, SEPTEMBRE, 1931

PROGRAMME

de la Réunion organisée par La Société d'Économétrie à l'Université de Lausanne, dans la Salle Tissot, les 22–24 septembre 1931

1re Journée, 22 septembre

Président: M. Boninsegni

10 h. — Allocution du *Recteur* de l'Université de Lausanne, M. Reymond.

Discours d'ouverture, par M. Frisch.

Cournot et l'école mathématique par M. Roy.

Walras, par M. Boninsegni.

14 h. 15 Méthodes nouvelles de mesurer l'utilité marginale, par M. Frisch.

16 h. 15 Réception par l'Université de Lausanne, à l'Hôtel de la Paix.

20 h. 30 Knut Wicksell comme précurseur de l'économétrie, par M. Åkerman.

L'économie mathématique et l'école autrichienne d'économie politique, par M. Weinberger.

Un contradicteur italien de J. B. Say sur l'emploi de l'algèbre en économie politique, par M. Del Vecchio.

Un économiste mathématicien du xviiime siècle: le général Lloyd, par M. Sraffa.

Détermination de la période économique en économie individuelle, par M. Rosenstein-Rodan.

2me Journée, 23 septembre

Présidents: MM. Del Vecchio et Fanno

10 h. — Détermination statistique des courbes d'offre et de demande, exposé général, par M. Marschak.

14 h. 15 Trois notions de l'équilibre économique, par M. Bousquet.

Les corrélations des prix et les courbes statistiques de demande et d'offre, par M. Fanno.

La demande dans ses rapports avec la distribution des revenus, par M. Roy.

La recherche de revenus équivalents en différents pays, par M. Staehle.

Distributions statistiques rattachées à la loi de Gauss et la répartition des revenus, par M. Darmois.

20 h. 30 Les systèmes autoentretenus et les oscillations de relaxation, par M. LE CORBEILLER.
L'emploi des équations aux différences dans l'étude des distributions statistiques, par M. FRISCH.

3me JOURNÉE, 24 SEPTEMBRE

Président: M. DARMOIS

10 h. — La statistique et les théories scientifiques, par M. DARMOIS.
Utilisation des équations fonctionnelles et des nombres complexes dans les recherches économiques, par M. TINBERGEN.
Le calcul tensoriel comme moyen de formulation invariante des lois économiques et statistiques, par M. FRISCH.
14 h. — Déjeuner en commun.
Discours de clôture, par M. FRISCH.
Après le déjeuner, excursion dans les environs de Lausanne.

Secrétaire de la réunion: M. Staehle, pendant la réunion, Hôtel de la Paix, Lausanne.

COMPTE RENDU DES SÉANCES

SÉANCE D'OUVERTURE, DU MARDI, 22 SEPTEMBRE 1931, À 10 H.

M. REYMOND, Recteur de l'Université de Lausanne, offre la bienvenue à la réunion de la Société d'Économétrie; il rappelle Walras et Pareto, et souhaite un travail efficace au congrès qui va s'ouvrir. Il exprime les sentiments cordiaux de l'Université de Lausanne.

M. FRISCH fait le discours d'ouverture:

Monsieur le Recteur, Messieurs, et chers collègues,

La réunion qui va s'ouvrir aujourd'hui est la première réunion en Europe de la Société d'Économétrie. Comme vous le savez il était prévu que M. Schumpeter prononcerait le discours d'ouverture. Malheureusement, il est subitement tombé malade et ne peut pas venir. Nous serons donc privés du grand plaisir qu'il eût été de l'entendre. Pour des raisons de famille notre vice-président M. Divisia a aussi été empêché de participer à cette réunion pour l'organisation de laquelle il a travaillé avec tant de dévotion. Veuillez accepter, Messieurs, cette remarque comme une explication du fait que c'est sur moi qu'est tombée l'obligation agréable de remercier de l'aimable accueil qu'on nous a fait ici à Lausanne, de vous dire un mot de bienvenue de la part du Conseil de la Société, et de donner un bref exposé, préparé à la dernière minute, sur l'idée de la Société d'Économétrie.

Comme vous le savez, les dernières décades ont été une période d'accumulation d'observations statistiques sur une échelle telle que nous ne l'avions jamais connue. Non seulement l'activité des bureaux statistiques officiels a-t-elle beaucoup accru, mais encore nous avons vu surgir une multitude d'institutions semi-officielles et privées qui ont pour objet d'observer statistiquement les divers aspects de la vie économique. Au point de vue matériel il semble ainsi que nous sommes en train de réaliser le rêve de *Stanley Jevons*. Jevons a entrevu un temps où les renseignements sur le caractère *numérique* des phénomènes économiques auront atteint un tel degré de perfection que les économistes seront en état de faire entrer l'élément numérique concret dans les cadres de leur théorie, et ainsi de constituer l'économie en une véritable science.

Toutefois, quiconque connaît la situation telle qu'elle existe actuellement en économie politique, sait que nous sommes encore très loin de réaliser le rêve de Jevons. Cela tient à deux choses. D'abord—il faut l'avouer—les recherches économiques théoriques pendant les dernières décades ne se sont pas inspirées autant qu'on aurait pu le désirer de l'évolution ininterrompue des institutions économiques et sociales. La théorie pure n'a pas réagi assez spontanément à la nouvelle situation concrète, mais a continué de dévouer une trop grande partie de son énergie à ce type de problème que nous avons hérités du siècle dernier. Pour ne prendre qu'un seul exemple, considérons le groupe de problèmes théoriques que l'on peut classer sous le titre: la dynamique économique. Ces problèmes sont d'une importance vitale pour l'explication des cycles économiques et plus généralement pour ces phénomènes où nous voyons surgir la différence caractéristique entre le système capitaliste et le système communiste. Une étude approfondie de ces problèmes dynamiques est à peine commencée. Tout cela montre que l'appareil théorique est encore trop rudimentaire, trop simple pour pouvoir recevoir et utiliser d'une façon vraiment effective les nouvelles données statistiques qui sont concrètes et compliquées comme la vie elle-même.

D'un autre côté bon nombre de ces nouvelles investigations statistiques ont été conçues d'une façon trop empirique. Ce n'est pas trop dire, je pense, que souvent on s'est laissé entraîner par une sorte de manie d'information statistique. On a manqué la sage faculté de juger entre le fondamental, le secondaire, et le superflu, que seul un esprit habitué à penser d'une façon théorique et systématique possède. En un mot, c'est la collaboration intime et suivie entre l'observateur et le penseur qui a manqué en économie politique, et qui fait que nous ne sommes pas encore prêts à réaliser le rêve de Jevons.

C'est ici que la Société d'Économétrie voit sa mission. Notre but

principal est de faire avancer autant que nos forces le permettent la collaboration de l'observateur et du penseur en économique. Nous aimons à formuler les lois de l'économie politique de telle façon que nous pouvons espérer, aujourd'hui ou demain, de les rendre concrètes en déterminant effectivement leur caractère numérique par un appel aux procédés statistiques.

Notre objet n'est dirigé contre personne. Le champ de l'économique est immense et il y a de la place pour des travailleurs de types différents. Nous respectons toute méthode et tout point de vue à une seule condition: celle de la sincérité.

Pourtant, notre but étant de formuler autant que possible les lois d'économie politique d'une façon quantitative, c'est surtout à trois aspects de notre discipline que nous devons nous attacher d'une façon particulière. C'est d'abord la tentative d'élaborer, de généraliser, et de préciser, la théorie économique elle-même. Il faut construire de nouveaux cadres pour interpréter de nouvelles données. Ensuite, c'est la technique des procédés statistiques qu'il faut développer en vue de l'emploi spécial que nous nous proposons. Finalement—avouons-le franchement et sans tremblement de cœur—la relation voulue entre la théorie économique et les réalités statistiques, nous ne pourrons l'établir sans le secours des mathématiques. La signification de ces trois points de vue est bien formulée dans le statut de la Société, tel qu'il a été adopté à la réunion d'organisation de la Société, tenue à Cleveland, Ohio, aux États-Unis, au mois de décembre de l'année dernière. Voici la formule: *An International Society for the Advancement of Economic Theory in its Relation to Statistics and Mathematics.*

Messieurs, au nom du Président et du Conseil de la Société d'Économétrie, j'ai l'honneur de vous souhaiter une bienvenue cordiale à cette réunion. Nous ne sommes qu' un très petit groupe, mais c'est un groupe actif, à juger d'après le programme qui est composé de toute une série de conférences et communications hautement intéressantes.

Notre réunion va se tenir en un lieu qui est devenu illustre dans les annales de l'économie politique. Nous remercions de tout notre cœur le Recteur et les autorités universitaires de l'Université de Lausanne, ainsi que le Département de l'Instruction Publique du Canton de Vaud, de l'aimable accueil et des grandes facilités qu'ils ont bien voulu nous accorder. Nous espérons que l'histoire scientifique de ce lieu sera une inspiration de plus dans nos travaux.

Messieurs, j'ai l'honneur de déclarer la réunion ouverte.

Le Secrétaire (M. STAEHLE) annonce la présidence de M. BONINSEGNI pour la premier journée.

M. BONINSEGNI prend la chaire.

Le Secrétaire annonce le programme de la journée. Il communique

la décision du comité d'organisation, que le français et l'anglais seront admis comme langues officielles, c'est-à-dire qu'aucune traduction ne sera donnée des communications faites en ces deux langues. Si les présents désirent se servir d'autres langues, une traduction sera donnée autant que possible. (Toutes les communications et les dicussions de la réunion ont été faites en français.)

M. Roy fait sa communication sur *Cournot*.

M. Boninsegni fait sa communication sur *Walras*.

M. Del Vecchio affirme que Pareto est un des auteurs les plus étudiés en Italie.

M. Frisch dit que c'est seulement par manque de rapporteur que Pareto n'a pas été fait l'objet d'une communication spéciale. Originalement, une telle communication était prévue dans le programme.

M. Roy dit que l'étude de Pareto est très cultivée en France.

M. Weinberger dit de même pour l'Autriche.

La séance est levée.

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22 septembre 1931, Séance de l'après-midi à 14 h. 15

Président: M. Boninsegni

M. Frisch fait sa communication sur les "Méthodes nouvelles de mesure de l'utilité marginale."[1]

M. Bousquet rappelle que M. Aupetit doit être mentionné dans la bibliographie de l'utilité. Il demande si l'on peut comparer les utilités marginales des nègres à New Orleans avec celles des blancs de Buffalo.

M. Frisch répond que la méthode de translation tient compte d'une telle variation tant que cela peut être fait par une correction apportée au prix de la vie et au prix du bien de comparaison.

M. Rosenstein-Rodan dit que l'économie pure veut expliquer les actes économiques. Mais est-il nécessaire de mesurer les motifs, et l'individu agit-il d'accord avec ces grandeurs? Kant déjà nie que l'individu se laisse guider par la mesure de valeurs intensives. L'orateur cherche à déterminer si le départ de l'utilité *pondérée* n'implique pas déjà la mesurabilité. Il faudrait éclaircir ce point.

MM. Boninsegni et Frisch disent que la position de Frisch n'est pas attaquée par ces considérations. La théorie des lignes d'indifférence est suffisante pour la théorie statique, mais non pas pour la théorie dynamique. Pour celle-ci il faut un mensuration directe de l'utilité.

M. Frisch se déclare d'accord avec la nécessité d'éclaircir les fondements de la logique de l'utilité. Il entre dans la discussion de l'axiomatique de la mensuration, parle d'axiomes locaux et interlocaux.

[1] Pour compte rendu de cette communication voir *Revue d'Économie Politique*, janvier 1932.

M. MARSCHAK propose une autre manière de dériver la surface de consommation. Il propose de baser la recherche sur *deux* relèvements budgétaires faits en deux périodes différentes.

M. DEL VECCHIO demande si l'on ne pourrait pas employer la courbe d'interpolation déjà utilisée par Engel pour simplifier la procédure.

M. FRISCH remarque qu'en partant des expressions d'Engel on impliquerait des hypothèses dont on ne voit pas les conséquences.

LE PRÉSIDENT lit une *proposition de résolution de M. Bousquet:*

La première réunion européenne de la Société d'Économétrie,

Après avoir entendu en sa première séance l'exposé de M. le Professeur Boninsegni sur l'œuvre de Walras,

Considérant que cette œuvre est une de celles auxquelles on ne saurait assez rendre hommage,

Considérant qu'il convient de commémorer le centenaire de sa naissance en 1834,

Emet le vœu que la célébration de ce centenaire soit préparée au plus tôt par le bureau de la Société pour être discutée en une prochaine assemblée.

LA RÉUNION *adopte* cette résolution à l'unanimité.

LA RÉUNION se rend à l'Hôtel de la Paix, où une réception lui est offerte par l'Université de Lausanne. Au cours de cette réception, qui occupe le reste de l'après-midi, M. DEL VECCHIO prend la parole pour remercier l'Université de Lausanne de l'accueil chaleureux qu'elle a bien voulu faire à la Société d'Économétrie. M. REYMOND, Recteur de l'Université de Lausanne, dit la grande joie que le choix de Lausanne comme lieu de la première réunion européenne de la Société fait éprouver à l'Université, consciente et orgueilleuse de la grande influence exercée en économie politique par Walras et Pareto.

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22 SEPTEMBRE 1931, SÉANCE DU SOIR À 20 H. 30

Président: M. BONINSEGNI

M. ÅKERMAN fait sa communication sur *Knut Wicksell.*

M. BOUSQUET demande si Wicksell, comme Schumpeter, nie la possibilité de l'intérêt en économie statique.

M. ÅKERMAN dit que non.

M. ROSENSTEIN-RODAN dit qu'il n'y a pas de différence de fond entre Schumpeter et Boehm-Bawerk, en ce qui concerne la théorie de l'intérêt. Il indique que Fanno et Mises se sont inspirés des idées de Wicksell.

M. ÅKERMAN dit que Wicksell a en vue différents genres d'oscillations (longues et courtes).

M. FRISCH pense que les oscillations dont parle Wicksell durent de 8 à 10 ans.

M. MARSCHAK pense que le rajustement de l'équilibre ne peut pas être expliqué par la simple adaptation de l'intérêt à la vitesse du mouvement des prix, provoquée par une différence entre le taux naturel de l'intérêt et le taux du marché.

M. FANNO montre que les perturbations temporaires du marché sont une exception aux théories de Wicksell. En automne le taux monte, au printemps il descend. Dans ces cas les prix ne changent guère, et leur mouvement (s'il y en a eu) s'arrête, même si la différence entre les deux taux continue pendant un certain temps.

M. WEINBERGER fait sa communication sur "L'économie mathématique et l'école autrichienne d'économie politique."

M. BOUSQUET pense que le nom d'école psychologique est mal choisi.

M. MARSCHAK parle de 'verstehende' et 'empirische' économie et psychologie. L'introspection est 'verstehend.' L'adoption de cette différence ferait disparaître toutes ces dicussions.

M. ROSENSTEIN-RODAN précise les différences entre Vienne et Lausanne. Lausanne ne s'occupe pas de la voie par laquelle on veut arriver du point initial au point final. Vienne s'intéresse à la voie.

M. DEL VECCHIO fait sa communication sur "Un contradicteur italien de J.-B. Say sur l'emploi de l'algèbre en économie politique."

M. SRAFFA fait sa communication sur "Un économiste mathématicien du XVIII^me^ siècle: le général Lloyd."

M. ROSENSTEIN-RODAN fait sa communication sur "La Détermination de la période économique en économie individuelle."

M. BONINSEGNI dit que l'analyse de M. Rosenstein ne tient pas suffisamment compte du milieu, dans lequel l'individu vit.

M. BOUSQUET par contre se déclare d'accord avec la façon de voir de M. Rosenstein.

M. MARSCHAK remarque que l'idée de tenir compte du groupe des "Besoins qu'on ne connaît pas d'avance" peut être importante pour le calcul de la vélocité de la circulation monétaire.

M. FANNO remarque que l'idée de Rosenstein peut servir à expliquer pourquoi avec le progrès économique on substitue l'épargne en nature par l'épargne en monnaie, et pourquoi à présent, qu'il y a une grande incertitude dans les conditions futures, les épargnants préfèrent tenir leurs épargnes liquides plutôt que de les investir.

M. FRISCH propose la comparaison avec la théorie de Boehm-Bawerk sur la 'perspektivische Verkürzung.'

La séance est levée.

23 SEPTEMBRE 1931, SÉANCE DE LA MATINÉE À 10 H. 30.

Président: M. DEL VECCHIO

M. MARSCHAK fait sa communication sur "La Détermination statistique des courbes d'offre et de demande, exposé général."

M. FRISCH demande des éclaircissements sur la formule de Marschak:

$$q = f\left(\frac{E}{p}\right).$$

Il ne voit pas pourquoi on fait l'hypothèse que p et P sont proportionnels.

M. MARSCHAK dit qu'on peut le supposer, si la dépense pour la marchandise considérée est une quantité peu importante dans le budget de famille. La réaction ne sera en général pas exactement la même si le revenu augmente de 10 pour cent ou si le prix spécial diminue de 10 pour cent. Mais l'erreur sera petite si la marchandise n'est pas importante.

M. FRISCH dit qu'alors (E/p) n'est pas le revenu réel. Mais alors on pourrait tout simplement dire que c'est un paramètre descriptif du problème. On pourra écrire ainsi:

$$q = f\left(\frac{E}{p}\right); \text{ où } px = q; \text{ et par conséquent } x = \frac{1}{p} f\left(\frac{E}{p}\right).$$

M. MARSCHAK accepte cette façon de transcrire sa formule.

M. STAEHLE se concerte avec M. Marschak contre le trend. Rien n'est gagné en exprimant la 'tendance séculaire' comme fonction du temps, parce que ce qui importe, c'est de savoir pourquoi il y a eu une tendance (séculaire ou autre). Il faut donc rechercher ses causes, et substituer une explication en termes de cause à effet à un traitement purement formel. Il propose d'abandonner toute tentative de rechercher la courbe de l'offre, puisque Cournot a montré déjà en 1863 que tout *déplacement* de la courbe de l'offre doit se présenter comme un mouvement sur la courbe de la demande, et que tout déplacement de la courbe de la demande doit se présenter comme un mouvement sur la courbe de l'offre. Il sera donc suffisant de connaître la courbe de la demande et ses déplacements pour expliquer les phénomènes du marché.

M. WEINBERGER ne comprend pas la critique de Marschak contre Moore, quant à l'usage des trend ratios dans ses équations walrasiennes.

M. MARSCHAK dit que les trend ratios sont superflues puisque les variables perturbatrices sont toutes inclues dans les équations. Il com-

pare ensuite ses résultats (élasticités) pour le sucre avec ceux obtenus par Schultz (0.4 et 0.5). Il explique les différences par les shiftings de l'offre et de la demande qui se sont produits dans le temps de l'observation de Schultz.

La séance est levée.

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23 SEPTEMBRE 1931, SÉANCE DE L'APRÈS MIDI À 14 H. 15

Président: M. DEL VECCHIO

M. BOUSQUET fait sa communication sur les "Trois notions de l'équilibre économique."

M. BONINSEGNI s'oppose à l'indétermination de l'équilibre prétendue par Bousquet. Il explique le cas de plusieurs racines, ainsi que la possibilité de racines imaginaires dont nous ignorons la portée. Mais l'équilibre n'en est pas moins déterminé.

M. WEINBERGER rappelle que dans la terminologie de l'école autrichienne le mode de la distribution des richesses est un fait donné.

M. LE CORBEILLER dit que selon Divisia il manque une équation dans la théorie statique de Pareto.

M. ROY apporte quelques précisions sur le point expliqué par M. Boninsegni.

M. FRISCH dit que selon Divisia c'est dans le cas où l'on introduit une marchandise indirecte que se manifeste le manque d'une équation chez Pareto. Il fait remarquer que la courbe des revenus n'est pas une condition supplémentaire.

M. FANNO fait sa communication sur "Les corrélations des prix et les courbes statistiques de demande et d'offre."

M. BONINSEGNI se félicite que M. Fanno ait montré la nécessité de tenir compte de toutes les équations de l'équilibre.

M. ÅKERMAN parle des courbes d'offre et de demande pour les capitaux et du taux de l'intérêt.

M. MARSCHAK remarque que des *corrélations* entre les prix des biens de remplacement, calculées sur des séries *temporelles* peuvent contenir des erreurs à cause d'autres influences perturbatrices (changement des goûts, par exemple).

M. DEL VECCHIO étant obligé de quitter Lausanne, c'est M. FANNO qui se charge de la présidence.

M. ROY fait sa communication sur "La demande dans ses rapports avec la distribution des revenus."

M. BOSQUET demande si en compliquant les hypothèses on ne pourrait pas arriver à de meilleurs résultats.

M. ROY espère que quelqu'un veuille se charger de cette recherche ultérieure.

M. BOUSQUET demande sur quelle période les données utilisées par M. Roy s'étendent et s'il ne faut pas considérer l'augmentation du revenu total nominal.

M. ROY dit qu'il a pu supposer la constance des autres circonstances.

M. MARSCHAK demande quelle est la dérivation de la formule pour λ (élasticité).

M. ROY en explique quelques détails et renvoie à son mémoire dans *Metron*.

M. MARSCHAK démontre qu'il doit exister des $\lambda > 1$ et communique des élasticités supérieures à l'unité qu'il a trouvées pour les Etats-Unis.

M. WEINBERGER ne voit pas de contradiction entre la loi de Gossen et la première hypothèse de M. Roy.

M. FANNO demande s'il ne s'est pas produit de déplacements des voyageurs vers les classes plus basses de billet de chemins de fer et si ce fait n'a pas eu d'influence sur les résultats de M. Roy.

M. ROY l'affirme, mais puisqu'il s'est servi seulement des données sur les recettes non classées, il n'a pas pu faire de recherches sur ce déplacement.

M. STAEHLE fait sa communication sur "La recherche de revenus équivalents en différents pays."

M. FRISCH demande diverses précisions et présente une autre méthode pour la détermination de revenus équivalents dans différents pays qui fait application des fonctions de flexibilité du revenu dont il a parlé hier.

M. DARMOIS fait sa communication sur "Une nouvelle formule des distributions statistiques et son application à la distribution des revenus."

M. TINBERGEN attire l'attention sur les travaux de J. van der Wijk ("Psychisch inkomen en psychisch vermogen," *De Economiste*, 1931, pp. 112, 232 et 305) qui a traité le même sujet.

M. FRISCH pense que la formule présentée n'est pas nouvelle.

M. BONINSEGNI tire des conclusions philosophiques de l'hypothèse développée par M. Darmois et dit qu'alors la distribution des revenus ne serait qu'un produit du hasard.

La séance est levée.

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23 SEPTEMBRE 1931, SÉANCE DU SOIR À 20 H. 30

Président: M. FANNO

M. LE CORBEILLER fait sa communication sur "Les systèmes auto-entretenus et les oscillations de relaxation."

M. FRISCH fait communication de sa correspondance avec M.

Schumpeter dans laquelle il a proposé deux modèles mécaniques de 'cycles.'

M. MARSCHAK parle de l'asymmétrie entre l'information et la conduite des consommateurs d'un côté et des entrepreneurs de l'autre.

M. LE CORBEILLER explique les analogies mécaniques de M. Frisch et l'asymmétrie des cycles du point de vue de sa communication.

M. WEINBERGER souligne l'importance des analogies comme instrument scientifique.

M. FRISCH donne un modèle illustrant des chocs aléatoires et en communique les équations.

M. LE CORBEILLER donne un modèle de relaxation (gouttes tombant d'une façon aléatoire dans un vase oscillant). Il promet d'envoyer aux membres des exemplaires de sa conférence donnée à l'Institut des Arts et Métiers.

M. FRISCH fait sa communication sur "L'emploi des équations aux différences dans l'étude des distributions statistiques."[2]

Il n'y a pas de discussion. La séance est levée.

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24 SEPTEMBRE, SÉANCE DE LA MATINÉE À 10 H.

Président: M. DARMOIS

Le Secrétaire fait des communications techniques. Le Comité du Protocole est composé ainsi: MM. Boninsegni, Fanno, Darmois, Tinbergen, Frisch, Staehle.

M. FRISCH fait une communication au sujet de la communication de M. Marschak d'hier.

M. BONINSEGNI intervient pour attaquer la notion d'un 'composé' de toutes les marchandises consommées, à part du tabac.

M. HAGSTROEM dit qu'il ne s'agit que d'un modèle abstrait. On peut supposer qu'il n'y a que deux marchandises.

M. FRISCH termine sa communication, indiquant une erreur dans le développement de M. Marschak.[3]

M. MARSCHAK réplique en admettant que M. Frisch a raison de dire qu'une erreur est commise, mais il se demande s'il n'existe pas un autre moyen de développer l'approximation en question.

M. FRISCH ne croit pas que le coup mortel a été donné à la méthode et qu'il sera possible de trouver un tel moyen.

M. LE CORBEILLER dit que la question etudiée dépend de la courbure de la surface.

[2] Voir *Metron* 1932.

[3] Pour un compte rendu complet, voir la *Revue d'Économie politique*, janvier 1932.

M. FRISCH remarque que selon lui il n'est pas question de la courbure de la surface, mais de son inclinaison au point étudié.

M. MARSCHAK dit que le problème, comment la grandeur de l'erreur, c'est-à-dire la différence entre les deux fonctions f et ϕ dans la communication de M. Frisch peut être exprimée comme une fonction de la grandeur relative de la dépense par rapport au budget total, est encore à résoudre. Il remarque que l'Institut für Konjunkturforschung de Berlin admet inconsciemment dans ses recherches que les biens plus élastiques réagissent plus sensiblement aux mouvements des salaires (et non seulement au mouvement des prix) que les biens inélastiques.

M. DARMOIS, convaincu de la vitalité montrée par la Société d'Économétrie, renonce à sa communication sur "La statistique et les théories scientifiques" qui est une justification désormais superflue. (La communication sera publiée).

M. TINBERGEN fait sa communication sur "L'utilisation des équations fonctionnelles et des nombres complexes dans les recherches économiques."

M. FRISCHE demande des explications sur la généralité des solutions de M. Tinbergen et communique une solution qu'il a trouvée pour un problème semblable. La solution donnait lieu également à des mouvements périodiques.

M. TINBERGEN dit qu'il a tâché en vain d'établir une ressemblance entre ses développements et les oscillations électriques.

M. LE CORBEILLER donne à cet égard la bibliographie suivante: Carson, *Operational Calculus and Circuit Analysis*, (New York), et Van der Pol, *Philosophical Magazine*, 1928 and 1929, où l'on trouve deux articles sur ce sujet.

M. FRISCH fait sa communication sur "Le calcul tensoriel comme moyen de formulation des lois économiques et statistiques."

M. MARSCHAK demande si la difficulté de manque d'invariance persiste lorsqu'on se sert d'échelles logarithmiques pour les coordonnées.

M. FRISCH dit qu'alors on a la difficulté d'un changement d'origine. La régression orthogonale de Pearson est invariante seulement pour une transformation orthogonale. Frisch a donné en 1929 une régression invariante pour une transformation homogène quelconque.

M. DARMOIS demande si la matrice métrique intervenant dans la théorie de Frisch ne sera pas la métrique de la loi des erreurs.

M. FRISCH répond que cette question ne peut être tranchée avant que l'on trouve la matrice métrique qui donne l'invariance (si une telle matrice existe).

La séance est levée.

DÉJEUNER EN COMMUN À L'HÔTEL DE LA PAIX, LE 24 SEPTEMBRE, À 14 H. 15

Après le déjeuner sont discutées diverses question relatives à la Société d'Économétrie.

M. STAEHLE fait des propositions relatives aux manuscrits des communications présentées à cette réunion.

M. HAGSTROEM veut faire une proposition d'ordre pratique. L'économie métrique ou économétrique n'est pas une science absolument nouvelle, comme il a été dit par MM. Boninsegni, Del Vecchio, Sraffa et Åkerman, mais les ouvrages qui ont paru ne sont ni très nombreux, ni trop étendus. On se trouve donc dans une situation particulièrement heureuse pour la formation d'une bibliographie rétrospective et surtout courante. La valeur de cette bibliographie serait augmentée considérablement si l'on pouvait obtenir de chaque auteur un petit résumé du contenu central de sa publication (une "Selbstanzeige"). Cette bibliographie serait spécialement précieuse pour les mathématiciens qui ne peuvent donner qu'une petite partie de leur temps aux questions intéressants la Société. C'est au nom des mathématiciens un peu étrangers aux questions économiques, comme les experts-comptables, les électriciens, et les mécaniciens, qu 'il veut faire cette proposition.

On discute la possibilité d'avoir des fonds pour la distribution des extraits d'articles des membres, ou pour l'établissement d'un bureau qui en serait chargé.

M. STAEHLE remarque que tout membre doit être un collaborateur actif dans la bibliographie et qu'il sera utile de constituer une ou plusieurs bibliothèques des publications des membres.

La discussion suivante porte sur la date, le lieu et le programme de la prochaine réunion européenne. Elle aboutit aux résultats suivants:

1. Les réunions doivent avoir lieu chaque année.

2. La date devrait être choisie dans la première semaine d'octobre, c'est-à-dire après la fin de septembre, mois généralement occupé par de nombreux autres congrès scientifiques, et avant le 10 octobre, date où commencent les examens dans de nombreuses universités.

3. Le lieu de la prochaine réunion devra être choisi lors de chaque réunion.

4. Pour l'année 1932, on a émis le vœu que la réunion ait lieu à Paris entre le 1er et le 7 octobre. D'autres propositions, à savoir Vienne, Zürich (Congrès des mathématiciens, proposé par M. Hagstroem) et Cambridge furent discutées, mais écartées.

5. Aucun programme très défini ne devrait être fixé d'une année à l'autre. Toutefois, la proposition de M. Frisch, de réserver une journée

destinée à la communication de nouveaux résultats obtenus par les membres pendant l'année (théoriques ou concrets) est acceptée. A la suite d'une remarque de M. Marschak, il est recommendé d'organiser à chaque réunion des "conférences-leçons" où les parties des mathématiques nécessaires aux études économétriques seront traitées.

6. La formation de Sections distinctes serait contraire à l'esprit même de la Société.

7. Sur proposition de MM. Fanno et Marschak, il est reconnu désirable d'inscrire au programme de la réunion de Paris en 1932 une journée réservée aux questions de la monnaie et du crédit.

M. Frisch prononce le discours de clôture.

M. Boninsegni remercie les organisateurs de cette réunion pour avoir choisi Lausanne comme lieu de la première réunion européenne de la Société d'Économétrie. Il se félicite des résultats obtenus. Il exprime sa reconnaissance à M. Frisch pour la grande dévotion aux buts de la Société, et formule son admiration pour M. Roy. Ensuite, il fait ses meilleurs souhaits pour l'avenir de la Société.

La séance est levée.

Le secrétaire de la réunion,
Hans Staehle

THE MEETING OF THE ECONOMETRIC SOCIETY IN WASHINGTON, D.C., DECEMBER, 1931

THE FIRST American meeting of the Econometric Society was held in Washington, D.C., December 28 and 29, 1931, in connection with the annual meetings of the social science societies.

PROGRAM

DECEMBER 28. MORNING SESSION

Joint meeting with the American Economic Association,
New Washington Hotel

ROUND TABLE CONFERENCE ON QUANTITATIVE ECONOMICS

Chairman, IRVING FISHER

The Absorption of Credit by Buying. JAMES H. ROGERS, Yale University

Quantitative Factors in the Distribution of the Value Product of Industry. MALCOLM C. RORTY, American Telephone and Telegraph Co.

The Relationship between Statistical Price Analysis and Mathematical Economics. MORDECAI EZEKIEL, U. S. Bureau of Agricultural Economics

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December 29, 1:00 P.M. Informal luncheon at the New Washington Hotel

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DECEMBER 29. AFTERNOON SESSION

Joint meeting with the American Statistical Association,
New Washington Hotel

Chairman, FREDERICK C. MILLS

Some Demands of Economics and Forecasting on Mathematical Economics. HARRY C. CARVER, University of Michigan

The Measurement of Monetary Phenomena. CARL SNYDER, Federal Reserve Bank, New York City

Some Relations between Commodity Prices and Business Activity. HOLBROOK WORKING, Stanford University

A Comparison of the Elasticities of Demand for Selected Commodities, Obtained by Different Methods. HENRY SCHULTZ, University of Chicago

REPORT OF THE MEETINGS

DECEMBER 28. MORNING SESSION 10:00 A.M.

A round table conference on "Quantitative Economics," a joint meeting of the American Economic Association and the Econometric Society, was held on Monday morning, December 28, 1931, at 10 o'clock in the United States Chamber of Commerce Building in Washington, D.C. The Chairman of the conference was Professor Irving Fisher, President of the Econometric Society. The following men presented papers: Professor James Harvey Rogers, of Yale University, on "The Absorption of Credit"; Colonel Malcolm C. Rorty, of the American Telephone and Telegraph Company, on "Quantitative Factors in the Distribution of the Value Product of Industry"; and Dr. Mordecai Ezekiel, of the United States Bureau of Agricultural Economics, on "The Relationship between Statistical Price Analysis and Mathematical Economics."

Professor Rogers' paper was a mathematical development and extension of the work of Chester A. Phillips, who had shown that an addition to its reserve by an individual bank did not, as was popularly supposed, result in increasing that individual bank's deposit liability by ten-fold (assuming the ratio of reserves to demand liabilities to be ten per cent); that, instead, the increase of reserve and increase of deposits were substantially equal; and that the pyramiding occurred through the overflow from the individual bank to the other banks. He pointed out that "in a period of general liquidation, plentiful money is an entirely different thing from easy money."

Professor Ray B. Westerfield, of Yale University, and Dr. C. O. Hardy, of the Brookings Institution, contributed to the discussion of Professor Rogers' paper.

Colonel Rorty's paper discussed the fundamental distribution between return for labor (personal service), capital, and natural resources. He arrived at the following conclusions: "(a) Scientific management is insisting on a higher and higher rate of capital turnover in business and industry as a whole. (b) The rate of improvement in machine tool equipment . . . is forcing a higher capital turnover in those very industries where an extreme building up of capital equipment might be expected. (c) The observed tendency toward a decline in the share of capital probably represents a real trend toward stabilization at a level lower than thirty per cent. . . . (d) Our economic and social philosophy may very reasonably be based on the assumption that capital, in the future, will receive less, rather than more, than thirty per cent of the total amount available for distribution between capital and labor."

This paper was discussed by Dr. E. J. Working, of the Bureau of Agricultural Economics in Washington, D.C., and by Professor Zenas C. Dickinson, of the University of Michigan.

The third, and final paper was that of Dr. Ezekiel on "The Relation between Mathematical Economics and Statistical Price Analysis." "The finds of one rest on facts, with a pinch of hypothesis," he said, "of the other on hypotheses which contain a pinch of facts. Neither adequately treats the non-regular and non-rational elements in economic life. . . . Each can contribute certain things to the other—mathematical economics, things to look for and relationships to run down; price analysis, tentative slope of economic relations, or dynamic demand and supply curves, which present material for further mathematical elaboration." But Dr. Ezekiel concluded that "a full generalized system is still far in the distance and may become increasingly distant—in spite of Moore's brilliant unification on non-realistic materials."

The discussion of Dr. Ezekiel's paper was started by Dr. Wassily Leontief, at present with the National Bureau of Economic Research in New York City, and by Dr. Warren M. Persons, of the Standard Statistics Company in that city.

December 29. Afternoon Session 2:30 p.m.

A joint session of the Econometric Society with the American Statistical Association was held on Tuesday afternoon, December 29, 1931, at 2:30 p.m. at the New Washington Hotel. Professor Frederick C. Mills, of Columbia University, presided as chairman. Four papers were read, namely, by Professor Harry C. Carver, of the University of Michigan, on "Some Demands of Economics and Forecasting on Mathematical Economics"; by Dr. Carl Snyder, of the Federal Reserve Bank of New York, on "The Measurement of Monetary Phenomena"; by Professor Holbrook Working, of Stanford University, on "Some Relations between Commodity Prices and Business Activity"; and by Professor Henry Schultz, of the University of Chicago, on "A Comparison of the Elasticities of Demand for Selected Commodities, Obtained by Different Methods."

Professor Carver's paper presented some of the demands of the economist and the forecaster for greater precision in the measurement of economic relationships. Forecasts must be accompanied, he said, "by their probable errors or some other index of their reliability." He also pointed out that "mathematical statisticians may be called upon to develop a new theory of probable error to meet the demands of time series analysis." Professor Carver gave an example of greater precision in measurement in his trapezoidal analysis of seasonal variations. This

simple method appeared, in some cases at least, to give more precise results than do some of the more complicated methods. Professor Carver's paper also contained a brief discussion of the problem of smoothing and of sampling as applied to time series.

Dr. Horace Secrist, of Northwestern University, and Dr. Frederick V. Waugh, of the United States Bureau of Agricultural Economics, opened the discussion of Professor Carver's paper.

In his paper on "The Measurement of Monetary Phenomena," Dr. Snyder defined over-production and under-production as deviations from the secular trend of production, which for all commodities for the world has been rising "at a singularly even rate of 4 per cent" since 1850 at least. Dr. Snyder showed that the war and post-war periods have been periods of under-production rather than over-production, and that "the commodity price level is related neither to the total of production nor to its variations from the line of normal growth." The collapse of prices in 1929, according to Dr. Snyder, seems to have been precipitated by the contraction of purchasing power, demand, and markets, rather than by production above trend.

Dr. Snyder's paper was discussed by Professor Ray B. Westerfield, of Yale University, and by Dr. Willford I. King, of New York University.

The third paper presented was that of Professor Working. In summary, Professor Working concluded (1) "that prices of wheat, and of farm products as a group, are 'sensitive' to the influences that cause cyclical fluctuations in commodity prices, and yet have been less influenced than wholesale prices in general by factors specifically related to industrial and speculative activity, or possibly influenced in the opposite direction; (2) that cyclical movements of commodity prices in general have been most significantly related to movements of prices of industrial stocks nine months earlier and to the timing of stock price movements relative to changes in industrial activity."

This paper was discussed by Dr. Warren M. Persons, of the Standard Statistics Company.

The final paper was "A Comparison of the Elasticities of Demand for Selected Commodities, Obtained by Different Methods," by Professor Schultz. This was a preliminary report on a series of elaborate statistical calculations of demand curves, or rather surfaces, in relation to time as a chief variable.

In view of the length and technical nature of this paper, it was not followed by discussion.

IRVING FISHER

Yale University

THE MEETING OF THE ECONOMETRIC SOCIETY IN NEW ORLEANS, LOUISIANA, JANUARY, 1932

THE Econometric Society met jointly with Section K of the American Association for the Advancement of Science in New Orleans, Louisiana, January 1932.

PROGRAM

FRIDAY MORNING, JAN. 1, 9:30 A.M.

Joint meeting with Section K of American Association for the Advancement of Science, Room G, Roosevelt Hotel

Chairman, GRIFFITH C. EVANS

A Dynamical Theory of Cost. CHARLES F. ROOS, Smithsonian Institution Building, Washington, D. C.

Economic Standards of Quality. W. A. SHEWHART, Bell Telephone Company

Edgeworth's Taxation Paradox and the Nature of Demand Functions. HAROLD HOTELLING, Columbia University

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FRIDAY NOON, JANUARY 1, 1:00 P.M. LUNCHEON, LA LOUISIANE RESTAURANT

The Scope of Econometrics. IRVING FISHER, Yale University. Business meeting and reports

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FRIDAY AFTERNOON, JANUARY 1, 2:30 P.M.

Joint meeting with Section K of American Association for the Advancement of Science. Room G, Roosevelt Hotel

Chairman, HAROLD HOTELLING

The Distribution of Chi-Square. EDWIN B. WILSON (and MARGARET M. HILFERTY) Harvard School of Public Health

The Rôle of Hypothesis in Economic Theory. GRIFFITH C. EVANS, Rice Institute

Some Pseudo-Scientific Methods in Economics. JOSEPH MAYER, Congressional Library

Certain Erratic Tendencies in Accountants' Income Data. JOHN B. CANNING, Stanford University

REPORT OF THE MEETING

JANUARY 1. MORNING SESSION, 9:30 A.M.

The Econometric Society met jointly with Section K in New Orleans, Louisiana, on the morning and afternoon of January 1, 1932. Three papers were presented at the morning session, at which Professor Griffith C. Evans, of Rice Institute, presided.

The first, by Dr. Charles F. Roos, Permanent Secretary of the Association, was entitled "A Dynamical Theory of Cost," and reformulated the static theory of cost in terms of dynamics. This paper was discussed by Dr. Joseph Mayer, of the Congressional Library, Professor Harold Hotelling, of Columbia University, Professor Irving Fisher, of Yale University, and others.

Dr. W. A. Shewhart, of the Bell Telephone Company, presented the second paper, discussing "Economic Standards of Quality." He pointed out the advantages of standardization and demanded a fairer basis for competition. Professor Hotelling and Professor Henry L. Rietz, of the University of Iowa, opened the discussion.

The following paper was that of Professor Hotelling on "Edgeworth's Taxation Paradox and the Nature of Demand Functions," which discussed certain surprising results of two interrelated demands when one of them was subjected to a tax. The discussion of this paper was led by Dr. Mayer and Professor Rietz.

JANUARY 1. LUNCHEON SESSION 1:00 P.M.

At noon, a luncheon meeting was held at which Professor Fisher, President of the Econometric Society, described its origin and objects. The following papers from Europeans were read by title:

1. "Flux et Stock," by François Divisa, (To be published in ECONOMETRICA)

2. "Pitfalls in the Statistical Construction of Demand and Supply Curves," by Ragnar Frisch, (To be published in *Frankfurter Gesellschaft für Konjunkturforschung*)

3. "Cio' Che e' Chiaro e Cio' Che e' Oscuro nelle Fluttuazioni dei Prezzi," ("Clear and Dark Aspects in Price Fluctuations") by Luigi Amoroso, (Published in Vol. 4 of the *Atti dell' Istituto Nazionale delle Assicurazioni*)

4. "The Demand for Rival (or Substitute) Commodities," by Umberto Ricci, (To be published in ECONOMETRICA)

5. "Kostenanalyse als Grundlage einer statistischen Ermittlung von Nachfragekurven," by Erich Schneider. (Published in *Archiv für Sozialwissenschaft*)

JANUARY 1. AFTERNOON SESSION 2:30 P.M.

Professor Hotelling presided at the afternoon session, four papers being presented. The program began with a paper by Professor Edwin B. Wilson and Margaret M. Hilferty, of Harvard University, on "The Distribution of Chi-Square." This paper was a brief statement of the distribution of chi-square in comparison with the normal probability curve.[1] The discussion which followed was extemporaneous.

The second paper was that of Professor Evans on "The Rôle of Hypothesis in Economic Theory," which treated the various uses and results of employing economic hypotheses. Professor John B. Canning, of Stanford University, Professor Wilson, Professor Fisher, and others took part in the discussion.

Dr. Mayer then presented his paper on "Some Pseudo-Scientific Methods in Economics." He suggested that classical dogmas regarding value be abandoned and that in social study we utilize the socio-psychological disciplines. Dr. Mayer's paper was discussed by Dr. Roos and Dr. Shewhart.

The final paper was that of Professor Canning on "Certain Erratic Tendencies in Accountants' Income Data," which discussed whether there is such a thing as a "constant expense" in enterprise operations, and attributed part of the amplitude of business cycle swings to belief in erratic income data. Professor Evans started the discussion which continued extemporaneously.

IRVING FISHER

[1] Published in *Proceedings of the National Academy of Science*, Dec. 1931.

THE MEETING OF THE ECONOMETRIC SOCIETY IN SYRACUSE, NEW YORK, JUNE, 1932*

By Joseph Mayer

The June, 1932, meeting of the Econometrica Society was held in conjunction with the summer gathering of the A.A.A.S., with which the Society is affiliated. Although held at an "off" season, it was the consensus of opinion among those in attendance that with respect to the numbers present, the scope of stimulating topics covered in the fifteen excellent papers offered, the high calibre of the ensuing discussions, and the splendid fellowship evidenced among scholars working at those outposts where mathematics, statistics, and economics meet, the results could hardly have been surpassed. The meeting lasted three days and covered six sessions, in addition to a luncheon meeting, the last session being devoted to the inspection of a statistical laboratory at Cornell University. Professors Ragnar Frisch and W. E. Mosher presided at the Tuesday morning and afternoon sessions respectively, and Professor Harold Hotelling at those on Wednesday and Thursday.

The *first day* was given over to two symposia, on "Demand and Supply" in the morning, and on "Statistics in the Social Sciences" in the afternoon. Three papers were presented at the morning session: "The Statistical Law of Demand for Producers' Goods as Illustrated by the Demand for Iron and Steel," by Roswell H. Whitman, of the University of Chicago; "The Effect of Hysteresis and Speculation on Demand and Supply," by C. F. Roos, of Washington, D. C.; and "An Experimental Study of Indifference Curves," by L. L. Thurstone, of the University of Chicago.

Mr. Whitman's paper concerned itself with the fitting of demand functions to price data for iron and steel. Terms involving the rate of change of price and the time-integral of price were included in various combinations. High multiple correlations were obtained for each of the periods 1902–1915, 1916–1920, and 1921–1930. The methods employed were in part those used by agricultural economists, and in part those suggested by Professors G. C. Evans and C. F. Roos. The following general equations were employed:

$$(1) \qquad y(t) = ap(t)+b+h\frac{dp}{dt}+\sum_{n=-\infty}^{0}\omega_n p(t+n) \begin{cases} p\text{: price} \\ \sum\omega_n p(t+n)\text{: past prices} \end{cases}$$

* In preparing this account at the request of the Editor of Econometrica, the writer is indebted to the excellent reports on various portions of the meeting by Harold Hotelling, Harry S. Kantor, S. S. Wilks, and F. G. Crawford.

$$(2) \quad y(t) = ap(t)+b+h\frac{dp}{dt}+cI+kt \qquad \begin{cases} I: \text{ industrial activity} \\ t: \text{ time} \end{cases}$$

Prices three, six, and nine months previous, were used as past prices, so that the concrete equations and the results of the study appeared as follows:

$$(1) \quad y(t) = ap(t)+b+hp'+a_1p(t-3)+a_2p(t-6)+a_3p(t-9)$$

period	a	b	h	a_1	a_2	a_3	R'	e
1902–15	-2.50 ± 1.00	4.58	$3.78\pm.64$	2.06 ± 1.28	$.02\pm.86$	$-1.07\pm.57$	.68	.46
1916–20	$-.60\pm.25$	4.56	$.78\pm.21$	$.49\pm.33$	$-.26\pm.15$		.67	.62
1921–30	-1.93 ± 1.09	$-.68$	$5.39\pm.99$	3.57 ± 1.28	$-1.26\pm.61$	$.83\pm.46$	.75	.56

$$x_1 \qquad x_2 \qquad x_3 \qquad x_4 \quad x_5$$

$$(2) \quad y(t) = ap(t)+b+h\frac{dp}{dt}+cI+kt$$

		R'	e
1902–15	$x_1=4.20-(1.56\pm.31)x_3+(7.99\pm1.06)x_4+(.036\pm.005)x_4+(.41\pm.12)x_5$	.81	.45
1916–20	$x_1=3.84-(.55\pm.08)x_3+(.48\pm.21)x_3+(.12\pm.011)x_4+(.15\pm.04)x_5$	.88	.39
1921–30	$x_1=1.49-(1.27\pm.23)x_3+(6.27\pm.94)x_3+(4.64\pm.35)x_4+(.03\pm.17)x_5$	.92	.41

In the above tables R' and e are, respectively, the coefficient of multiple correlation and the standard error of estimate corrected for the number of parameters. It will be noted that the coefficient of p is negative, as required by economic theory, and that the coefficient of p' is positive, showing that rising prices have a stimulating effect on demand, and falling prices the reverse. Other factors may be significant also in changes of demand, such as the timing of the cycles in the separate series.

An extended discussion was provoked by Mr. Whitman's paper. Dr. Harry S. Kantor, of New York, objected that some of the coefficients obtained were too small in comparison with their standard errors and excessively variable from period to period. The latter objection was amplified by Dr. Ragnar Frisch, of the University of Oslo, Norway, who spoke of the indeterminateness which may arise in a least-squares solution when the determinant of the normal equations is small. Professor Harold Hotelling, of Columbia, insisted that neither of these statistical objections appeared to affect Mr. Whitman's results: the latter were admittedly tentative in view of the pioneer nature of the undertaking; the determinant of the normal equations appeared in the denominator of the expression for the probable error of the unknown, and any excessively small value for this determinant would become immediately evident in an excessive probable error; and the fact that the coefficients varied from period to period, like so many other economic quantities, could hardly be held to invalidate their applicability to the periods for which they were respectively obtained. Professor Frisch replied to Professor Hotelling that a small value for the deter-

minant of the system will not, in general, be indicated by large probable errors of the regression coefficients. These probable errors will themselves become indeterminate in the case referred to by Professor Frisch. Professor Frisch regretted that this fundamental difficulty inherent in the entire classical correlation approach had not, so far, received the attention it merited. He pointed out the necessity of a systematic study of statistical cluster types, and the importance of using in this study characteristic parameters that—like the square of the collective alienation coefficient used by Professor Frisch—are polynomials in the total correlation coefficients, and, therefore, do not easily become indeterminate. Dr. Louis H. Bean, of the U. S. Department of Agriculture, questioned the significance of the terms involving the derivatives and integrals, since he had made a study of the iron and steel industry with the help only of algebraic regression equations and had obtained reasonable results. Dr. Roos pointed out that the variations among Mr. Whitman's coefficients for the different periods might well be due to such extraneous matters as currency conditions, and that the general equation used was intended primarily for refined analyses of periods only about a year in length.

In the second paper of this session, Dr. Roos (whose earlier work had in large part resulted in the formulation of the general equation used by Mr. Whitman) set forth equations of a still more general type to show more fully how the effect of past prices and the memory of past purchases lead to a demand function containing an integral, either of past prices or of past purchases, the two forms being equivalent on account of the invertibility of the linear integral equations. Starting with an equation of the type

$$y(t) = f[p(t_1), p(t_2), \cdots p(t_n), t]$$

and assuming only linear relationships, certain mathematical operations reduce it to the more usable form

$$y(t) = a_n p(t_n) + b + a_1(t_1)p(t_1) + \cdots a_{n-1}(t_{n-1})p(t_{n-1}).$$

The transformation

$$k(x, t) = na_1(t),\ t_1 \leqq x < t_2$$
$$k(x, t) = na_2(t),\ t_2 \leqq x < t_3$$
etc.

then gives the integral equation

$$y(t) = ap(t) + b\int_{t_1}^{t} k(x, t)p(x)dx.$$

The equation is also shown to be invertible; and the influence of speculation (i.e., of future prices) is introduced by writing

$$y(t) = b(t) + \lambda\int_{t_1}^{a+t} k(x, t)p(x)dx$$

which allows for estimates of future prices. Further work in process by Dr. Roos is expected to result in a "new dynamical theory of demand

in which the utility theory will be demoted from a major to a very minor position" in economic theory.

In the discussion Dr. Joseph Mayer, of Washington, D.C., stressed Dr. Roos' contentions regarding the relatively decreasing importance of the utility concept, and the influence of habit and customary price upon present price. He pointed out that the long-recognized relationship between demand and price, both for market demand and for individual demand, can be deduced without recourse to utility theory. Assuming constant utility for all, the market demand curve would still be negative because of differences in income, which contingency might be designated as a "law of diminishing income." For the individual demand curve, the influence both of buying habits and of habit in the rate of consumption must be taken into account and a distinction should be drawn between acquisitive demand and demand for consumption. A "law of prudential buying"—the aim to make the dollar go as far as possible—can account for the individual's negative demand curve quite apart from utility considerations. Dr. Roos' paper was also discussed by Dr. Mordecai Ezekiel, Professors Harold Hotelling, H. T. Davis, and Ragnar Frisch, who commented upon the difficulties encountered with characteristic values, the problem of solution for the Fredholm and Volterra equations in Roos' set-up and the fruitfulness of an approach which regarded hysteresis and speculation as the same under certain conditions. Dr. Ezekiel pointed out that the influence of speculation does not depend on future events, but on business judgment based on objective knowledge of past events.

In the final paper of the session, an experimental study of indifference curves was described by Professor Thurstone. Each of a number of subjects had been asked to choose deliberately between certain combinations of possessions, such as six hats and two pairs of shoes in comparison with four hats and three pairs of shoes. From the results were obtained indifference curves whose equations, when expressed in terms of the logarithms of the quantities as coordinates, were linear. Fechner's law ($S = k \log x + c$), which for two commodities gives $x_1^{k_1} \cdot x_2^{k_2} = m$ (satisfaction), was utilized. Hats, shoes, and overcoats, were the test commodities. With respect to each standard (e.g., four hats and four pair of shoes), a number of choices were indicated as falling in the second and fourth quadrants (considering the standard as origin), the indifference curves representing the results.

In the discussion, Dr. Ezekiel pointed out that for the purposes of economics a distinction must be made between answers to questions and actual market behavior. Housewives' answers, for example, indicated an elastic demand for milk, while objective studies showed the demand to be inelastic. Dr. Mayer continued the discussion by em-

phasizing the distinction between order of preference and measure of preference. He suggested that, while one may tentatively accept judgments of "more or less" satisfaction, where indicated by choices under set conditions, the actual quantitative measurement of satisfactions is quite another matter. The latter, because of differences in desires between individuals and of fluctuation in desires within a given individual, is a highly elusive undertaking and certainly impossible of accomplishment along the lines of classical deductions. As he has indicated elsewhere, these contain much fallacious reasoning. Dr. Thurstone concluded the discussion by pointing out that his study was not concerned with conduct, but was rather an attempt to find out whether any order can be discovered in a simplified study of choice.

Two papers were given at the Tuesday afternoon session: "Statistics and Political Science," by Herman C. Beyle, of Syracuse University; and "Statistics and Psychology," by L. L. Thurstone, of the University of Chicago. Professor Beyle, in the opening paper, concerned himself with making a plea for the development of what he called a *situational* political science, as opposed to customary practice. He mentioned census data, returns from official voting, and records of administrative operations, as types of situational materials lending themselves to statistical treatment of most constructive import for political science. This discipline deals with individuals in overt action, political situations as such being reducible to the following categories: (a) *Multiple-individual* situations (political, governmental, or legal); (b) Situations involving directly or indirectly a past, present, or future *legal rule;* (c) Situations involving an official status, particularly a *sovereign* status.

Professor Thurstone, in his paper, extended the methods of Spearman, illustrating his results with three examples. He first described a study of the interrelations among psychotic symptoms, in which the presence or absence of each of a list of such symptoms was tabulated for several hundred patients. After collecting complete data for 37 of the symptoms, it was found that five factors are sufficient to account for the observed interrelations. All insane patients could thus be classified according to the following broad insanity catagories: Tantrums, hallucinations, depressions, refusal of food, and defective reasoning. As a second illustration, the attempt was made to define personality traits in terms of a relatively small number of types, using as a basis several hundred adjectives in common use for the conversational description of personality. A small number of orthogenal axes (four or five) are set up and each adjective is designated as a point in n-dimensional space, the principal axes being thought of as unit personality vectors. In order to get a unique solution for this problem, the axes

must be so placed that the sum of the squares of the projections of all the points on the principal axes is maximized. As a third problem calling for multiple factor methods, Professor Thurstone selected vocational interests utilizing the Strong vocational interest schedule, which is elaborate and expensive to score. A careful study of Strong's data showed that his correlations of the interests of 19 professions could be accounted for by postulating only four factors: interest in science, interest in language, interest in people, and interest in business. The multiple factor methods used by Professor Thurstone proved to be consistent with the two-factor problem of Spearman, since the latter's tetrad difference equation may be written as a special case of the multiple factor equation

$$r_{ab}=a_1b_1+a_2b_2+a_3b_3, \ + \cdots, \ +a_nb_n.$$

Impose Spearman's restriction that there shall be only one common factor.

Then if $a_2\neq 0, b_2=0$ Or if $b_2\neq 0, a_2=0$
if $a_3\neq 0, b_3=0$ if $b_3\neq 0, a_3=0$

so that

$$r_{ab}=ab,$$

and by analogy

$$r_{ac}=ac,$$
$$r_{ad}=ad,$$
$$r_{bc}=bc,$$
$$r_{bd}=bd,$$
$$r_{cd}=cd,$$

hence

$$r_{ab}r_{cd}-r_{bc}r_{ad}=(ab)\ (cd)-(bc)\ (ad)=0$$

which is Spearman's tetrad difference equation.

The discussion of these two papers turned chiefly upon contentions of the speakers that ethics and human intervention have no place in social science, which should be treated in precisely the same way as natural science. Dr. Mayer, while admitting that geology, biology, anthropology, and psychology (as natural sciences), should be looked upon as providing the basis upon which social science builds, pointed out that the social structure is in large part man-made, that social "laws" should therefore be looked upon as more changeable than natural "laws," and that ethics and legal codes are part and parcel of the same type of inevitable human intervention in social affairs, which type of structural intervention is impossible in natural science.

The second day of the meeting was given over to two symposia and a luncheon meeting. The morning symposium was on "Mathematical Economics." The afternoon session was on "Agricultural Supply and Demand Functions."

Three papers were presented at the morning session, as follows: "On the Invariance of Linear Regressions," by Ragnar Frisch, of the University of Norway, Oslo; "The Perturbation Problem for Economic Series," by H. T. Davis, of Indiana University; and "Industrial Replacement," by A. J. Lotka, of the Metropolitan Life Insurance Company. A report on mathematics was also presented. The session was opened with Professor Ragnar Frisch's paper, which considered the problem of finding several types of linear regressions invariant for a general linear transformation, introduced the notion of a scatter space of n variables characterized by a general metric matrix, and showed that there exists no invariant regressions of a certain form in this space. Professor Frisch did, however, find several special invariant regressions, some of which have complex coefficients, and others which involve both linear and product moment functions of the observations.

The discussion of this paper was opened by Dr. S. S. Wilks, of Columbia University, who showed that the contribution of Professor Frisch can be connected with the theory of small samples, and described recent developments which had yielded the sampling distribution of determinants of covariances and of certain ratios of these determinants. This form of distribution, which has a very wide range of application and generalizes R. A. Fisher's analysis of variance distribution, aroused considerable discussion. Professor J. Shohat asked whether Dr. Wilks' solution is necessarily unique, in view of the fact that he obtained the distribution from its moments, since it is well known that over an infinite interval the moments do not determine the distribution uniquely. Dr. Wilks replied that the conditions given by Stekloff were satisfied in this case and that the solution was unique.

The second speaker Wednesday morning was Professor Davis, who presented a preliminary report in which he dealt with the computation and interpretation of three functions of economic variates. The first of these he called the function of economic state, the logarithm of which is the negative of a quadratic form in the variates. The second, which he called the function of economic variation, is a function similar to the time-derivatives of the variates. The third is a perturbation function which he defined as a quadratic in the time-derivatives. By setting up an analogy with physical systems he obtained a series of second-order differential equations and showed how the problem of perturbation in economic series is similar to that of explaining the methane spectrum by means of the perturbations of the atoms.

In discussing this paper Professor Frisch objected that Professor Davis' functions are not general enough to permit of a treatment of cyclical series. With respect to the elimination of trend, Professor Hotelling pointed out the advantages of using orthogonal polynomials,

which avoid unnecessary recalculation when it is decided to increase the degree of a trend line already fitted, save time on the fitting even of a fixed-degree curve, and moreover provide a much simplified interpretation of the sampling errors of the coefficients of the various terms, since these sampling deviations are all uncorrelated with one another.

The next part of Wednesday morning's program was devoted to a report on the kind of mathematics needed for work in the social sciences, adopted by the Social Science Research Council after the deliberations of a committee on the subject, and read by Dr. Ezekiel. It was recommended that undergraduates preparing for work in the social sciences should study logarithms, graphic methods, interpolation, equations and forms of simple curves, probability, the elements of differential and integral calculus, and curve fitting. Because of the time necessary, however, to acquire these mathematical tools in the usual school and college curriculum, it was recommended that shorter courses be organized, omitting solution of triangles in detail and other subjects of no importance to students of social science. For those planning to become professional statisticians or to do advanced research work in the social sciences a larger amount of mathematics would naturally be expected.

Professor E. R. Hedrick, of the University of California, led the discussion on the report, remarking that those interested in college mathematics should be greatly assisted, in teaching students of the social sciences, by the information therein presented. He approved of the proposal for shortened courses in mathematics for social science students, but added that it would be useless for mathematics departments to organize such courses unless the social science departments required their majors to take them, since experience demonstrated that students usually do not elect special courses of this sort. Professors Hotelling, Mayer, Carver, and Davis, continued the discussion along similar lines, and Professor Frisch closed with a plea for more general instruction in, and use of, higher algebra, including matrices, in connection with economics and statistics. After the discussion, a resolution endorsing the report was read by Dr. Ezekiel and unanimously adopted.

The last paper of this session, prepared by Dr. Lotka and read by Professor Hotelling, presented a mathematical approach to the problem of industrial replacement along lines previously considered by E. B. Kurtz on an empirical basis. The paper was supplemented by a model which illustrated the distributions-in-time of several generations of units for the case where the distribution of the first generation is a type I Pearson curve.

The members of the Society then met for luncheon at the Faculty

Club, at which Professor Hotelling spoke on "The Place of Statistics in the University." He pointed out that in addition to the usual two duties of the university professor, teaching and research, the mathematical statistician performs a third—responsibility of an advisory character, which takes a great deal of time, but is nevertheless of importance in suggesting new mathematical problems whose solutions are of value in a great variety of applications. It is highly desirable, he continued, that the next generation of statistics teachers be prepared for all three duties. Such preparation should include as much, or almost as much, mathematics as is now usually required for the doctorate in mathematics, an extensive knowledge of one or more subjects such as economics, psychology, biology, and engineering, and a thorough laboratory acquaintance with statistical tools.

Three papers were presented at the Wednesday afternoon session, as follows: "Supply-Price Relationships," by F. A. Pearson and G. F. Warren, of Cornell University; "Analyzing the Prices of Competing Products," by Mordecai Ezekiel, of the Federal Farm Board; and "Characteristics of Agricultural Supply and Demand Functions," by Louis H. Bean, of the U. S. Department of Agriculture.

Professor Pearson gave the results of a study of the relation between the supplies of corn, potatoes, wheat, and beef, in various localities of the United States, and prices (in some cases retail and in others wholesale prices) in New York City. The paper was commented upon by Dr. Mayer, who raised a question as to the definition of normality, and by Dr. Ezekiel, who pointed out the difference between a family of demand curves plotted as ratios of variates to normals, and a family obtained by plotting the raw values of the variates.

In the next paper, Dr. Ezekiel examined various functional set-ups for the demand curves for two competing products. In particular, he discussed the cases where the price of each product is expressed as an additive function of the supply of both, and where the price of each is expressed in terms of its own supply and the price of the other. He also considered various functions for obtaining a composite price. The paper was supplemented by several illustrative charts.

In the discussion, Professor Frisch indicated how the demand and supply of two commodities can be expressed in terms of the prices of both, and then introduced various confluent relationships corresponding to different assumptions about demand and supply. Professor Hotelling discussed the following set-up for n commodities:

$$\text{supply } \begin{cases} q_i = f_i(q_1, q_2, \cdots, q_n) \\ p_i = F_i(q_1, q_2, \cdots, q_n) \end{cases} \quad \text{demand } \begin{cases} q_i = g_i(p_1, p_2, \cdots, p_n) \\ p_i = G_i(p_1, p_2, \cdots, p_n) \end{cases}$$

for $i = 1, 2, \cdots, n$, and subject to the conditions of the form

$$\frac{\partial f_i}{\partial q_j}=\frac{\partial f_j}{\partial q_i}, \text{ etc.}$$

and pointed out the possibility of applying a generalized correlation coefficient to the problem of groups of non-independent commodities.

In the last paper of this symposium, Dr. Bean discussed the derivation of instantaneous demand-supply curves, characteristics of shifts in demand curves, and a type of demand-supply curve which represents the response of producers to price in future planning. All of these properties were illustrated with agricultural data. In the discussion, Dr. Frederick V. Waugh indicated that it is better for several reasons to use a demand-supply curve of the form $p=a+(b+cz)q+dz$ than one of the form $p=a+bq+cz$, where p is the price, q the quantity, a, b, c, and d are constants and z is business activity.

The third day was given over to a symposium in the morning on "Money, Credit, and Capital," and to the visit to Cornell in the afternoon. Three papers were presented at the morning session: "The Effect of Credit on Purchasing Power," by Thomas N. Carver, of Harvard; "The Concept of Momentum and Inertia in Economics," by Carl Snyder, of the Federal Reserve Board; and "A Theory of Interest from the Equation of Exchange," by M. D. Anderson, of the University of Florida.

Professor Carver outlined the manner in which the expansion and contraction of credit affect business and prices. He maintained that the cause of instability was an overstimulation of consumer's credit during a limited period of expansion. No matter how highly credit is developed, so long as it remains at a constant level, the consumers can purchase no more on credit than with cash. Men can use credit only to economize on gold and to enable the consumers to enjoy the goods a little earlier than would be possible if they waited until they paid cash. In summing up, he said that "this leaves us with the conclusion that consumers' credit does not, in itself, increase consumers' purchasing power. It is only during the period when such credit is actually expanding that there is any increase in purchasing power."

Dr. Snyder, the second speaker, illustrating the long-period movements of economic variables with elaborate colored charts, stated that for 200 years in the United States there had been an almost unchanging rate of population growth, and maintained that over a long period there are no cycles in world production, no variations in relative prices, and that prices of staple products, such as wheat, corn, and cotton, are determined by forces outside the product itself. It has always been held that when prices rise the demand for currency increases, but at the present time, we have a decline in prices concurrent with a stationary

or rising demand for currency. For the past century the importance of money as a medium of exchange has been decreasing, and the relation between business changes and amount of money in circulation has long since passed. It used to be thought that credit was affected by business. Now there is the clearest evidence that the converse is true and that it is business which is affected by the volume of credit. Dr. Snyder made a plea that the false theories which he said had been accumulating for 350 years be thrown aside to make way for a more realistic approach.

The third speaker, Professor Anderson, stated that the capital of a nation is limited not by what people save but by what they produce and consume, illustrating his contention with charts to show that the capital or wealth of the United States tends to be equal to its annual production. According to his view, an increase of production comes about largely through technological advances which, if they are to be worth while, require not a smaller, but a larger amount of consumption. Failure of consumption to increase in the same ratio nullifies the beneficial effect of greater physical volume of production by lowering values and producing a protracted depression.

Discussion on these three papers was animated, sharp issue being taken with some of the contentions of the speakers. Dr. Frisch and others took especial exception to Dr. Snyder's statement that most of the business-cycle analysis of the past has been fantastic. Dr. Alvin H. Hansen, of the University of Minnesota, maintained in discussing Professor Carver's paper, that expansion of consumers' credit is an effect rather than an initial cause; that the disturbing forces which cause oscillations of profits, output, and employment, are to be found in the dynamics of investment; that while consumption does indeed control the *direction* of investment, it does not control its *quantity*, which is determined by the forces which control the distribution of a nation's income over time—in short, by the rate of interest; and that, in brief, fluctuations in purchasing power arise, not from the consumer, but from fluctuations in investment, from changes in the rate of interest and the prospective rate of profit, from changes in technology, and from the condition of the capital market.

The Thursday morning session closed the meetings of the Society in Syracuse. After lunch, members motored to Ithaca, where, as guests of Cornell University, they were shown about the campus, including particularly the statistical laboratories presided over by Professor G. F. Warren and Professor F. A. Pearson. The meeting ended with a delightful picnic in Cascadilla Gorge.

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PAPERS TO APPEAR IN ECONOMETRICA

THE SURVEYS

Alvin H. Hansen: Significant Developments in Business Cycle Theory. (to appear April, 1933)

W. A. Shewhart: Significant Developments in Statistical Technique. (to appear July, 1933)

Jakob Marschak: Significant Developments in Statistical Information. (to appear October, 1933)

J. Tinbergen: Significant Developments in General Economic Theory. (to appear January, 1934)

OTHER CONTRIBUTIONS TO APPEAR IN EARLY ISSUES

Johan Åkerman: Knut Wicksell—A Pioneer of Econometrics.

Marco Fanno: Interrelations des prix et courbes statistiques de demande et d'offre.

Umberto Ricci: On the Demand for Rival (or Substitute) Commodities.

Mordecai Ezekiel: Some Considerations on the Analysis of the Prices of Competing or Substitute Commodities.

René Roy: La demande dans ses rapports avec la répartition des revenus.

Henry Schultz: A Comparison of the Elasticities of Demand for Selected Commodities, Obtained by Different Methods.

Ragnar Frisch: The Notion of Elasticity with Regard to a Given Definition System.

Joseph Mayer: On Certain Pseudo-scientific Methods in Economics.

G. H. Bousquet: Les trois notions de l'équilibre économique.

Irving Fisher: The Income Concept.

François Divisia: L'utilité d'une théorie générale des ensembles renouvelés.

G. Darmois: Distributions statistiques rattachées à la loi de Gauss et la répartition des revenus.

J. Shohat: On Interpolation.

Eugene Slutsky: The Summation of Random Causes as a Source of Cyclic Processes.

Gustavo Del Vecchio: Francesco Fuoco.

J. Tinbergen: Sur la théorie dynamique de l'échange.

L. Furlan: Le volume maximum du crédit dans le cas d'un système économique isolé.

A. L. Bowley: The Action of Economic Forces in Producing Frequency Distributions of Income, Prices, and Other Phenomena.

CONSTITUTION OF THE ECONOMETRIC SOCIETY

An International Society for the Advancement of Economic Theory in its Relation to Statistics and Mathematics

1. Scope of the Society

The Econometric Society is an international society for the advancement of economic theory in its relation to statistics and mathematics. The Society shall operate as a completely disinterested, scientific organization without political, social, financial, or nationalistic bias. Its main object shall be to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences. Any activity which promises ultimately to further such unification of theoretical and factual studies in economics shall be within the sphere of interest of the Society.

2. Members

The Society shall consist of two classes of members: regular members and Fellows. Members of either class shall be subject to election. To become a regular member, a person must be proposed to the Council by two members of the Society. Once a year the Council shall nominate new members and these nominations shall be voted upon by mail by all members. No person can be elected to membership unless he is nominated by the Council.

During the first year following the organization meeting of the Society, the Council shall have the authority to invite eligible persons to become charter members.

3. Fellows

All Fellows of the Society shall be nominated by the Council and elected by mail-vote of the Fellows. Such nomination may be made at any time. To be eligible for such nomination a person must have published original contributions to economic theory or to such statistical, mathematical, or accounting analyses as have a definite bearing on problems in economic theory, and must have been a member of the Society for at least one year. Each year the Council shall offer the members an opportunity to suggest nominees for fellowships.

The first group of Fellows shall be elected by the Council.

A meeting of the Fellows shall be held each year at a place and time

designated at the previous annual meeting of the Fellows. In default of action by the Fellows, the Council shall have the power to fix the place and date of such meeting. The annual meeting of the Fellows shall represent the highest authority of the Society. In important cases, the annual meeting of the Fellows may decide by a simple majority vote to subject any decision of the meeting to ratification by mail-vote of all Fellows.

4. Officers

The President of the Society shall be elected for one year by ballot by the Council. The President shall act as chairman of the Council.

The first President shall be elected by those present at the organization meeting of the Society.

The Council may elect one or more of its members to act as Vice-Presidents of the Society for a period of one year.

The Council shall elect a Secretary and a Treasurer to serve for a period of one year. These two offices may be combined. The Treasurer shall present financial statements to the Council, which, in turn, shall submit the statements to the annual meeting of the Fellows.

5. The Council

The Council shall consist of not less than seven nor more than fifteen members. The President, the Secretary, and the Treasurer, shall be ex-officio members of the Council. The other members of the Council shall be elected with a view to representing the various geographic areas in which the Society has members. These latter members of the Council shall be elected for three years by the Fellows upon nomination by the Council, the offices for approximately one-third of them terminating each year. The first Council shall be elected by those present at the organization meeting of the Society.

6. Activities

Any activities which fall within the sphere of interest of the Society may be authorized by the Council, such as local scientific meetings, international meetings under the auspices of the Council, and the issuance of publications reporting the activities of the Society or containing other matters of econometric interest.

7. Financial Organization

The dues for regular members and the dues for Fellows shall be fixed by the Council. Such dues shall be moderate in amount. Bequests and gifts may be received.

8. Amendment of the Constitution

Amendments to this Constitution must be approved by a simple majority vote of those Fellows present at any annual meeting of the

Fellows. Before becoming effective they must be ratified by a two-thirds majority of those voting in a mail-vote taken among all Fellwos.

* * *

This Constitution was adopted by general vote of those present at the organization meeting of the Society in Cleveland, Ohio, U. S. A., December 29, 1930.

The meeting decided to leave desirable verbal modifications in the text of the Constitution to be made by a committee consisting of Ragnar Frisch, Frederick C. Mills and Charles F. Roos. This committee held a meeting in Cleveland, Ohio, U. S. A., on December 30, 1930, and there decided upon the final phrasing.

Signed:

RAGNAR FRISCH FREDERICK C. MILLS CHARLES F. ROOS

REPORT OF THE WORK OF THE COUNCIL

AT THE organization meeting of the Econometric Society on December 29, 1930, in Cleveland, Ohio, U.S.A., the following persons were elected to the Council (in addition to the President, who is a member ex-officio):

Luigi Amoroso, University of Rome; L. v. Bortkiewicz, University of Berlin; A. L. Bowley, London School of Economics; François Divisia, École Polytechnique (Paris); Ragnar Frisch, University of Norway (Oslo); Charles F. Roos, Smithsonian Institution Building, Washington; Joseph Schumpeter, University of Bonn; E. B. Wilson, Harvard University; Wl. Zawadzki, Ministry of Finance, Warsaw. With the exception of Professor Bortkiewicz, who died in 1931, this Council has served for two years, these first two years being officially considered as one.

In 1931, the Council elected the Vice-President, Secretary and Treasurer, and 157 persons as Charter Members, in addition to the 16 persons present at the organization meeting. In 1932, it nominated to membership a list of others, the names of whom are now to be submitted to the members for their vote.

In January, 1932, it authorized the President and the American members of the Council to accept the generous offer of Mr. Alfred Cowles 3rd, for financing a journal for the Society; and afterward the Council voted approval of the schedule of dues.

In February, 1932, Professor Ragnar Frisch was elected Editor-in-Chief of the journal with power to select his own editorial board and assistant editor, and "ECONOMETRICA" was chosen as the name for this journal. Mr. Alfred Cowles 3rd, was elected Circulation Manager of the journal and also Treasurer of the Society.

At the request of Mr. Cowles, the Council selected the following five men to serve as an Advisory Council for the Cowles Commission for Research in Economics: A. L. Bowley, Irving Fisher, Ragnar Frisch, Carl Snyder, and W. C. Mitchell.

The Council authorized the President or Vice-President to arrange the following meetings of the Society:

Lausanne, Switzerland, September 22–24, 1931;
Washington, D.C., U.S.A., December 28, 29, 1931;
New Orleans, Louisiana, U.S.A., January 1, 1932;
Syracuse, New York, U.S.A., June 20–23, 1932;
Paris, France, October 1–4, 1932;
Atlantic City, New Jersey, U.S.A., December 27, 28, 1932; and
Cincinnati, Ohio, U.S.A., December 31, 1932.

August, 1932

IRVING FISHER

PROGRAMME DE LA RÉUNION DE LA SOCIÉTÉ D'ÉCONOMÉTRIE, PARIS, OCTOBRE, 1932*

SAMEDI Ier OCTOBRE

Président: M. C. COLSON

10 h. — Allocution de bienvenue par M. C. COLSON.
Discours d'ouverture par M. F. DIVISIA.
Communications: Analyse quantitative de l'offre et de la demande.
M. E. H. PHELPS BROWN: The Statistical Derivation of Demand Schedules: A Criticism.
M. ERICH SCHNEIDER: Construction statistique des courbes du prix de revient.

Président: M. O. BAÑOS

14 h. 30 *Communications: Questions théoriques d'ordre général:*
M. RAGNAR FRISCH: La notion d'élasticité relative à un système de définition donnée.
M. J. TINBERGEN: Sur la théorie dynamique de l'échange.
M. ARTHUR L. BOWLEY: The Action of Economic Forces in Producing Frequency Distributions of Incomes, Prices and Other Phenomena: a Suggestion for Study.
17 h. — Réception par le Comité à la Maison des Polytechniciens.
20 h. 30 *Colloquium*

DIMANCHE 2 OCTOBRE

Président: M. ARTHUR L. BOWLEY

10 h. — *Communications: Théorie du polypole.*
M. F. ZEUTHEN: Bilateral Monopoly (the fight between two bargaining parties)
M. ERICH SCHNEIDER: Polypoly of non-joint production.
M. RAGNAR FRISCH: Outline of a General Theory of Polypoly.

LUNDI 3 OCTOBRE

Président: M. MORTARA

10 h. — *Communications: Questions monétaires dans leur rapport avec la statistique.*
M. OLEGARIO BAÑOS: Construction pratique des nombres indices.
M. OSKAR LANGE: Some Remarks on The Concept of Purchasing Power of Money.
M. J. MARSCHAK: Vélocité du capital et vélocité de la monnaie.

* Compte rendu de cette réunion sera donné par M. Georges Lutfalla dans *La Revue d'Économie Politique*, avril, 1933.

M. Hans Staehle: Nouvelle note sur la recherche des revenus équivalents et sur la mesure du coût de la vie en différents pays ou en différentes périodes.

Président: M. J. Marschak

14 h. 30 *Communications: Théorie monétaire pure.*

M. Furlan: Le volume maximum du crédit dans le cas d'un système économique isolé.

M. A. Pikler: Les bases d'une théorie cinétique de la monnaie.

M. Hermann C. Scheibler: Le flux du pouvoir d'achat dans l'organisme économique.

M. Hans Bolza: Aspects monétaires de la crise.

20 h. 30 *Colloquium*

M. Gibrat: Une loi générale de création des courbes de fréquence dans l'économie politique.

Mardi 4 octobre

Président: M. Turlan

10 h. *Communications: Méthodologie et statistique.*

M. Raoul Husson: Critique des méthodes de Walras-Pareto dans l'économie politique.

M. Raffacle d'Addario: Courbes de fréquence dans les assurances.

M. Risser: Progrès récents de la statistique mathématique.

Président: M. Pirou

15 h. 30 *Communications: Questions diverses.*

M. Hamburger: Quelques considérations générales sur un système économique des équilibres.

M. F. Divisia: Quelques lacunes de la théorie classique de l'équilibre.

19 h. 30 *Dîner en commun* à la Maison des Polytechniciens.

*

Communications reçues par le Comité:

M. Gino Borgatta: La théorie de l'intérêt dans ses rapports avec les théories du revenu, du profit, et du salaire.

M. Chayrou: L'achat rationnel et les mesures d'utilité relative.

M. Gustavo del Vecchio: Les nouvelles théories monétaires.

M. Montgomery: Quelques propriétés des indices de prix d'un groupe de marchandises.

KNUT WICKSELL
1851–1926

KNUT WICKSELL, A PIONEER OF ECONOMETRICS

By Johan Åkerman

Paper read at the meeting of the Econometric Society, Lausanne, September, 1931

The progress of economic science from the *Tableau économique* of Quesnay up to the *Synthetic Economics* of Moore may be symbolized by a tree gradually growing higher. The tree trunk is strongly rooted and its nature was determined by the place where it grew.

Signs of a fresh orientation have become increasingly frequent during the last ten years, but, as yet, we do not see clearly which method to follow to unite the economics of yesterday to that of tomorrow. In the meantime, we can observe how minute inquiries into economic cycles, the laborious statistical determination of demand curves, and similar studies, are constantly opening up new perspectives of the fundamental economic problems.

The classical theory was essentially that of Ricardo, and any effort to build up a theory conforming to modern economic life and modern scientific conceptions must, therefore, be in conflict with the Ricardian edifice. The classical, Ricardian theory is an economic theory of static equilibrium. Any attempt to look for assumptions more true to economic life will more or less become an economic theory of Time. It is a remarkable fact that most economists who have attempted an exhaustive analysis of these economic factors, in which time is necessarily involved, have found themselves at variance with the classical economic theory of Equilibrium. Two names must be mentioned in this connection: Stanley Jevons and Knut Wicksell. In this short account of the latter I shall take up only a single aspect of his work, and try to show why and how he may be called a pioneer of econometrics.

With regard to Wicksell's life and work in general, I shall content myself with the brief statement that he was born in 1851 and died in 1926, and that his three principal works were published at the age of 42 to 47 years, that is to say, between 1893 and 1898: *Ueber Wert, Kapital und Rente* (Jena, 1893), *Finanztheoretische Untersuchungen* (Jena, 1896), and *Geldzins und Güterpreise* (Jena, 1898). He was a professor of Political Economy at Lund University, Sweden, from 1900 to 1916, and two volumes of *Lectures* appeared during this period, one of which is a theory of money (1906), and the other a theory of value based on the principle of marginal utility (1913).

A synopsis of Wicksell's econometric works can be given under the following five heads:

1. Theory of population.—Significantly enough, Wicksell, who is always searching for the essentials of economic life, begins his scientific work by studying the growth of population and its social consequences. Man is the centre of his investigation, man is the measure of every act of economic consequence. While the individual man is the kernel of the economic problem, Society is its natural boundary. Society as a whole is, thus, what Jevons has called a "trading body," and hence the typical member of it—the "economic man"—is really simultaneously a true individual and a representative of Society. By their jesting references to the activities of the human machine—the "economic" man—the adherents of the historical school have only succeeded in demonstrating that they have learnt nothing from the theory of Probability and nothing from experimental psychology.

Wicksell is, perhaps, Malthus's most remarkable successor. What Wicksell said several decades ago regarding the necessity of restricting the increase of population—drawing upon himself the persecution of conservative Society—is today generally accepted. His researches on marriage, birth, and death rates, and their economic and social consequences, opened up the possibility of calculating an optimum of population. At the present time, however, the problem of the optimum is not the same as at the time of Knut Wicksell's researches. The conceptions of economic relativity and economic dynamics have modified the problem.

Nevertheless, Wicksell gave to the theory of Population a new impulse which proved of capital importance. He demonstrated that for every estimate of an optimum of population a thorough knowledge of the whole economic mechanism is essential. And as that mechanism is put in motion by human requirements and opinions in respect to the relative utility of everything that can satisfy these requirements, the theory of Population must in the last instance rest on the theory of Value.

Wicksell's researches on population have thus stated the problem to be solved, and his mathematical training enabled him from the very first to perceive its great scope.

2. Theory of value.—A study of Wicksell's theory of Population shows that his theory of Value must be based on the conception of marginal utility. One might even say that the necessity of finding a basis for calculating the total utility of wealth of a population has led Wicksell towards the theory of Value, and notably to Menger's theory.

Many have taken pains to emphasize the differences between the Lausanne and the Austrian Schools. Knut Wicksell's theory of Value is the best proof that these differences have been much exaggerated, and that the attitudes of the two are essentially the same. Subjective value and marginal utility, the interdependence of production costs and rate of exchange, are their common fundamental principles.

Though Wicksell did not take sides with the Austrian school against that of Lausanne, he opposed those economists who tried to popularize Walras, by whom the truth of the Italian proverb "traddutore-traditore" was far too frequently illustrated. In a masterly paper he showed that Professor Cassel's system as a whole is an effort to trace back Walras's edifice to its classical source. Cassel's equations are thus superposed on the Ricardian theory minus any theory of value. Relying as it is on price as the general explanation of all economic phenomena, it is probably the most static system in existence.

That is the reason why Wicksell recognized the inapplicability of Cassel's system. We touch here on the connection between Wicksell's theory of Value and his theory of Capital. If, as we believe, all Wicksell's theory tends towards an economic theory of Time and not simply one of Equilibrium, capital—which necessarily has a time dimension, a duration—would naturally prove a difficulty, and that was just where Wicksell could no longer agree to Walras's ideas. Directly from this general point of view, Wicksell has, however, never criticized the Walrasian system. And there again, I think, the force of tradition prevented him.

Wicksell's theory of Value nevertheless borders on a theory of Capital; or it would be better to say that his theory of Capital has expanded into a general theory of Value. It is, in fact, a theory "Über Wert, Kapital und Rente."

Wicksell also applied his theory of Value to financial problems, notably to the question of the incidence of taxation and to the problems of monopoly, but it would take us too far to go into a detailed discussion of these questions.

3. The theory of capital.—How can we find a formula actually combining value and capital? In other words, how can we combine value and time? That is the first difficulty. Wicksell has often stated that Böhm-Bawerk's theory of Capital is the starting point of his own solution. Böhm-Bawerk, in turn, follows Jevons, who tells us that capital is that factor of production which helps us to dispense with labour in advance. Böhm-Bawerk's idea of the decision of entrepreneurs to begin new operations—to engage in roundabout ways of production—being the primary fact in the formation of capital, is the basis of Wicksell's explanation. If the time between the beginning and the end of an undertaking is the measure of the value of the new capital, if the marginal productivity of capital is determined by time, then time is the most important variable in the equation of production.

Wicksell's theory of Capital is, in fact, an economic theory of Time. We are here far from the static problem of division of profits, from the "Zurechnung" properly speaking. The primary question is not that

of division and momentary returns, but how production will *develop*, assuming Society to be trying to raise marginal productivity to the maximum.

A comparison between Professor Schumpeter's important investigations and those of Wicksell is extremely interesting. The former has shown that interest on capital is derived from entrepreneur profit, and, by assuming a static Society without interest on capital (if loans for consumption are disregarded), he found a contrast in progressive Society, where entrepreneur activity gives rise to interest on capital.

As Mr. Weinberger has demonstrated in his book, many other problems remain to be solved before the Austrian and the Mathematical schools can be amalgamated. Nevertheless this is undoubtedly the most important problem of the theory of Capital, and every advance here will, more or less, be an advance for both the schools.

4. Theory of money.—Quite consistently and logically, Wicksell discusses the theory of Money after the theory of Capital. The essential problems of production and consumption, of capitalization and exchange, should not be mixed up too early with the purely monetary problem. Wicksell was a strong adherent of the pre-war idea that economic relations cannot be clearly understood unless the veil of monetary factors is removed. It would have been fortunate if this fact had been more generally recognized in recent years; as a matter of fact, the policy of stabilizing the general price level, which so far has dominated the discussion, only deals with certain signs of cyclical variations—signs which during a period of inflation may truly indicate the essential causes of the variations, but at other times are of quite different import.

The fact that Wicksell realizes the importance of keeping money problems separated from the central problems enabled him to make a discovery of the first importance in the sphere of the theory of Money. This is his famous law of the interdependence of loan rate of interest, general price level, and profits of capital, found in his book of 1898.

Thornton had already given a lucid statement on the relation of profits to loan rate of interest, but to the very end of the nineteenth century this problem, like almost every other of an exact nature and requiring a time dimension for its development, was submerged under the classical theory of Equilibrium. Since the closing decade of the last century, however, this problem has been tackled independently by Wicksell, Irving Fisher, Hawtrey, and Mises. Wicksell is the forerunner among these, and the only one who discriminates between profit and loan rate of interest, and this is where his theory touches on the economic theory of Time. For the difference, positive or negative, between these two magnitudes is a function of time, in a word, an index of the

actual changes in economic life. One might say, therefore, that Wicksell returned to the classics in order to recover their clear comprehension of the *periodicity* of all changes, a comprehension which, since Ricardo's and Thornton's time, had been blotted out, both in subsequent commentaries and in the equations of the first mathematical school that centered around the strict theory of Equilibrium.

In another sense, also, one might say that Wicksell's law anticipated an economic theory of Time. While modern economists who had studied the relations of loan rate of interest to prices have actually used these relations as the starting point of their explanations of economic cycles, Wicksell refused to accept this monetary theory as a theory of economic cycles. According to him these obscure cycles are principally a result of the facts that the formation of capital cannot be a uniform process, that technical and commercial progress cannot advance at the same rate, and that consequently there must be periods of adaptation between supply and demand. Here we have a conception which is gradually becoming the central thesis of the theory of progress and economic cycles. It is also worthy of note that Wicksell has highly praised Cassel's theory of economic cycles in the last volume of his *Sozialoekonomie.*

According to the author himself, Wicksell's law thus applies only to secular periods, let us say to periods of fifty or sixty years. During a period of rising prices—like that from 1896 to 1913—the profits will always in the long run exceed the cost of loans—and there will be a surplus for the entrepreneurs which encourages them to increase production and capitalization. During a period of falling prices—like that from 1874 to 1895, and also after the post-war financial stabilization—the loan rate of interest will control capitalization. The cost of loans has, in the long run, a tendency to rise above the profits, which discourages a good many enterpreneurs from increasing production and capitalization.

The ultimate basis of such a theory is necessarily psychological, but in this connection I will only point out that Wicksell himself very distinctly observes that different laws apply to periods of different duration. Marshall, it is true, speaks of different economic periods in discussing the complex relations of supply and demand, but his discussion leads to static laws. The result in the long run will be of the same kind as the momentary result in a static society. It is, therefore, Wicksell whom we must consider as the true pioneer of a dynamic money theory during the last century.

5. Inductive and quantitative researches.—Wicksell's very strong sense of the limitations of all deductive reasoning saved him from ever falling into the error of dogmatism. He also felt the constant neces-

sity, which he had learnt in his early studies of mathematics and astronomy, of falling back, whenever possible, on scientifically observed facts.

It is above all very remarkable that as early as in 1908 Wicksell formulated a plan for the statistical study of cyclical fluctuations. He demonstrated that the variations in *stocks* of commodities are of very great importance, as during a depression they form a reserve of capital which—as Spiethoff had already asserted—is the basis of the new period of prosperity. He asked the Scandinavian industrialists and large wholesale merchants to provide information for continuous statistics on the variations of stocks. On this point Wicksell, the thoroughgoing theorist, is a forerunner of Mitchell, the pioneer of the inductive method.

Up to about fifteen years ago Wicksell was the only exponent of the quantitative and mathematical method of economic research in the Northern countries. And in recent years the paramount importance of his work is beginning to be understood by a large international public.

I cannot explain Wicksell's thought on the application of mathematics better than by quoting his dictum on Bowley's study *Mathematical Groundwork of Economics*. He says:

> Without sufficiently numerous and sufficiently exact statistical data, mathematics cannot be applied to any vast subject, but it is of the same service in the science of economics as everywhere else; it helps us to think. While it cannot take the place of intellectual labour,—efforts of that kind will always fail,—it is of excellent use in fixing the assumptions and conceptions on which to base our reasoning. Without this help, our assumptions and conceptions are apt to be transformed, or lost sight of, in long, complicated deductions, and at the end we frequently find no difference between premises and conclusion.

This short review will, I trust, show that Knut Wicksell may truly be called a pioneer of econometrics. He makes a clear distinction between pure and applied economics, and also between political economy, sociology, and economic history. In the theories of Population, of Capital, of Money, and of Economic Cycles, as well as in mathematical and statistical methodology, many economists now follow in his footsteps. The problems he has formulated are again being faced, and the scope of his ideas is now properly understood.

Knut Wicksell is thus a *modern* economist—an astounding assertion to make of a scientist who died, almost an octogenarian, five years ago. But it is the mark of a genius to be a generation in advance in the evolution of science.

Among Swedes who have contributed to the development of economic science, there is one outstanding name. That name is Knut Wicksell.

Lund University

ANNUAL SURVEY OF BUSINESS CYCLE THEORY:

INVESTMENT AND SAVING IN BUSINESS CYCLE THEORY

By Alvin H. Hansen and Herbert Tout

I

INTRODUCTION

The problem of the business cycle has been attacked from many angles. Recently the line of approach which has come into the greatest prominence is that which seeks to discover in the disequilibrium of investment and savings the causes of the fluctuations of business. Empirically, we know that the most salient characteristic of cyclical movements of business is the fluctuation in the production of capital goods. This fact presents a challenge to the economist, and the problems which it suggests have been attacked by methods as diverse as those of Spiethoff and Cassel, Aftalion and J. M. Clark, Schumpeter and Robertson.

In this paper we propose to limit ourselves to the recent contributions made by the investment and savings analysis. The great merit of this approach is that it provides a method through which it may be possible to integrate the monetary and the real phenomena which together underlie the business cycle. This possibility was clearly seen by Wicksell.

Foremost among the champions of the investment and savings approach are the economists of Vienna, led by Mises, but more recently represented by F. A. Hayek. In the English-speaking world the outstanding proponent of this particular method of analysis is John Maynard Keynes, who has arrived at his conclusions for the most part independently and without close knowledge of the works of the continental economists. It is in these contributions, and in the controversies which they have engendered, that we find the most signal recent developments of business cycle and of monetary theory.

The use made by Keynes and Hayek of the method above suggested is strikingly different, and the results which they achieve stand out in sharp contrast. Hayek concludes from his analysis that current attempts at management of the currency are likely to be dangerously disrupting. He remains a staunch champion of *laissez faire*. Keynes, on the other hand, is more firmly than ever the advocate of currency management and believes that he has discovered a weapon which will greatly aid the managers.

Perhaps the simplest way to envisage the fundamental causes of in-

dustrial fluctuations is to compare—admitting that analogies are always imperfect, if not dangerous—the economic mechanism to a pendulum. Here is a mechanism, which, by virtue of its inner structure, tends to oscillate back and forth once the state of equilibrium is disturbed by an outside force. It should be noted that the pendulum would not swing in cycles were it not for: first, the disturbing outside forces, and second, the peculiar internal structure of the mechanism itself.

A precisely similar statement may be made about the modern economic order, and it is basic to an understanding of the cycles of prosperity and depression. Business would not run in wave-like movements were it not for disturbing or initiating forces which destroy the equilibrium. Moreover, even granting the operation of these disrupting influences, cyclical fluctuations would not follow therefrom were it not for the peculiar *structure* of the modern economic order.

To understand the business cycle it is necessary first, to examine the internal structure of the present order, which "causes" economic life to move in a wave-like manner in the same sense that the internal structure of the pendulum "causes" it to swing in definite periods;[1] and second, to examine into those initiating forces which "cause" the cycle movement in the same sense that a kick "causes" the pendulum to swing.[2] Those characteristics of the internal structure, and those initiating forces which run in terms of *real* as opposed to *monetary* phenomena, will be familiar to students of Spiethoff. Yet while these

[1] A statistical attack upon the varying periods of replacement of the different forms of durable goods, whether consumers' or producers' goods, the changes in the length of these periods, which have come with the changing structure of industry and the development of new products, and the cumulation of these thousands of replacement cycles, each of different length and varying over time, into a single generalized replacement index, may be expected to throw light upon the recurrence of minor and major cycles, their relation to each other, and the varying lengths of the depression phase of the cycle in different historical epochs. Similarly, a statistical attack upon the varying periods of "gestation" of the different forms of durable goods may help to throw light upon the variations which are found in the length of the period of prosperity. A breakdown of the process of production, along the lines suggested, into component cycles or periods of gestation and replacement might well prove to be a most valuable statistical attack upon the business cycle problem. It should help to answer the question whether on the basis of technology any logical ground can be found for the apparent element of periodicity (if we may apply that word to a recurrent oscillation which varies in length) in business fluctuations, or whether the phenomenon may better be described as a "Konjunktur," a fortuitous combination of circumstances. It is quite possible, also that the periodicity of the cycle may be determined in part by the functioning of the institution of money, as Hawtrey believes.

[2] We are indebted for this analogy to Dr. Ragnar Frisch.

real factors would cause economic dislocations even in a barter economy, they do not adequately account for business cycles in a money and price system. The impact of technical change upon a capitalistic economy is magnified, in its disrupting effect, by passing through the money transformer.

Hayek appears to hold the extremely simplified view that the disturbances which continually arise from the side of *real* phenomena in a highly dynamic world could be prevented from causing serious errors of judgment on the part of entrepreneurs by the simple device of holding money neutral. Keynes, on the other hand, has too much faith in the occult powers of counter *monetary* adjustments to ease the frictions engendered by non-monetary disturbances.

II

KEYNES' THEORY

For the sake of clearness we shall describe the main variables entering into Mr. Keynes' theory, and indicate how his fundamental equations are obtained.

1. *Physical volume of production.*

O = Total volume of production (= the output as a whole).

R = volume of production of consumers' goods.

C = volume of production of new capital goods (investment goods). It is the net increment to the community's physical capital, and does not include the replacement of worn-out capital goods.

By these definitions we get

$$O = R + C$$

which may be called the volume-equation.

2. *Total money expenditure on production.*

E = total money expenditure in the production of the output as a whole (= total money earnings of the factors of production including normal remuneration to entrepreneurs, but excluding windfall profits. E is therefore the normal income of the community).

E_r = total money expenditure in the production of consumers' goods.[3]

I' = total money expenditure in the production of new capital goods (investment goods).

By these definitions we get

$$E = E_r + I'$$

[3] The symbol E_r is not used in Keynes' book but is introduced here.

which may be called the expenditure equation.

3. *Price levels.*

Π = price level of the output as a whole.

P = price level of consumers' goods.

P' = price level of capital goods (investment goods).

4. *Values of output.*

ΠO = total money value of the output as a whole.

PR = total money value of the output of consumers' goods.

$P'C = I$ = total money value of the net output of capital goods (of new investment goods).

5. *Savings and windfall profits.*

$S = E - PR$ = money value of savings, i.e. that part of the community's normal income which is not spent on consumers' goods.

$Q = \Pi O - E$ = windfall profits, i.e., sales proceeds of entrepreneurs minus all expenditures of production (these expenditures including, amongst others, normal remuneration to entrepreneurs).

Since windfall profits are no part of E, it follows from the definition of saving that windfall profits spent on consumers' goods are equivalent to negative saving. Savings and windfall profits, thus defined, are therefore spent on capital goods. Thus

$$I = Q + S.$$

This equation may be called the investment equation.

6. *Unit costs.*

E/O = cost of production per unit of the output as a whole.

I'/C = cost of production per unit of new investment goods.

The units of measurement of O and C are so chosen that, in the base year, unit cost of production is the same for capital goods as for the output as a whole. In other words, we have

$$\frac{I'}{C} = \frac{E}{O}.$$

This equation may be called the base-year equation.

7. *The two fundamental equations of price levels.*

By the definition of savings we have

$$PR = E - S.$$

Multiplying E by $\frac{R+C}{C}$ which, by the volume equation, is equal to unity, we get

$$PR = \frac{E}{O}(R + C) - S.$$

Multiplying out the parenthesis, and inserting I' for $C \cdot \frac{E}{O}$, according to the base-year equation, we obtain

$$PR = \frac{E}{O} \cdot R + I' - S.$$

Finally, dividing through by R, we get

$$P = \frac{E}{O} + \frac{I' - S}{R}.$$

This is the first fundamental equation.

The windfall profits Q are, on the one hand, equal to $\Pi O - E$ and, on the other hand, by virtue of the investment equation, equal to $I - S$. These two expressions must consequently be equal, which leads to

$$\Pi = \frac{E}{O} + \frac{I - S}{O}.$$

This is the second fundamental equation.[4]

* * *

Let it be observed at the outset that the first fundamental price level equation contains an error. For, in the derivation of this equation, use was made, as we have seen, of the base-year equation, which does not in general hold good for other years unless we suppose that the cost of production of a unit of producers' goods changes at exactly the same rate as the cost of production of a unit of consumers' goods. This was recently pointed out elsewhere by one of the authors.[5]

[4] The authors wish to express their thanks to Professor Ragnar Frisch who, in large part, formulated these definitions, which, it is believed, will be an aid to the reader of the critique which follows.

[5] Alvin H. Hansen, "A Fundamental Error in Keynes' 'Treatise on Money,'" *American Economic Review*, September 1932, p. 462. Mr. Adarkar has suggested that, since over short periods of time the relationship between the cost of production of units of producers' and of consumers' goods is unlikely to vary, the modification makes little difference to the theory. But if the equations are to serve their purpose in analyzing the business cycle, it is essential that this relationship be preserved during the crucial period of a cycle. Yet the fluctuations in cost of production of certain consumers' goods do not follow the same course as those of producers' goods. For this reason at some stage of the cycle, even in two consecutive short periods, I' might vary appreciably from $E(C/O)$. See *American Economic Review*, March, 1933.

Mr. Keynes in his reply[6] recognizes this error, and proposes two ways by which it can be eliminated. One method is to introduce a new factor k into the first equation. Thus E_r/R, the cost of production of a unit of consumers' goods, multiplied by k, is equal to I'/C, or the cost of production of a unit of producers' goods. k is thus a factor which is used to relate the efficiency earnings of the factors engaged in producing consumers' goods, E_r/R (which we will call W_r) to that of the factors engaged in producing producers' goods, I'/C, which we will call W_c. If we adopt this device, the equation becomes

$$P = \frac{E}{R + kC} + \frac{I' - S}{R}.$$

This modification makes the equation correct. However, in this case, the quantity C is somewhat arbitrarily introduced into the derivation of the first equation. It may well be that a better method of eliminating the error would be to avoid the introduction of the term C.[7] The equation could then be derived thus:

$$\begin{aligned} E &= E_r + I' \\ PR &= E - S \\ &= E_r + I' - S \\ P &= \frac{E_r}{R} + \frac{I' - S}{R} \\ &= W_r + \frac{I' - S}{R}. \end{aligned}$$

This, it will be noted, corresponds with the form of the second equation which Keynes writes as follows:

$$\Pi = W_1 + \frac{I - S}{O},$$

where $W_1 = \frac{E}{O}$ or the rate of earnings per unit of output.

Keynes' other device to eliminate the error is the one which he proposes to adopt in new editions of his book. It is to re-define the units in which the physical quantities are measured, as follows: "Let us choose our units of goods consumed as the quantity which had unit

[6] *American Economic Review*, December 1931, p. 690.

[7] We are indebted to Mr. Erik Lundberg, of Stockholm, for the suggestion of this method.

cost of production in the base year, and our unit of goods invested in any year as the quantity which has in that year the same cost of production as a unit of goods consumed." The objection to this method is a somewhat serious one. The output O must be conceived of as a physical and not as a value or cost concept. Yet it is difficult to do so if the units in which one part of O (namely C) is measured, are subject to change as the factor k fluctuates. It means that O, thus defined, might remain unchanged, while output measured by an invariable physical unit was actually changing. This makes it impossible to be certain that changes in O really reflect changes in physical output, and not merely changes in k. If, therefore, in subsequent revisions, Keynes re-defines the unit as indicated, and thus retains the equations as printed in the first edition, he will have made the second, as well as the first equation, of no practical significance, since the unit of measurement shifts from year to year.

This uncertainty as regards the definitions of C and O is perhaps typical of the difficulty of making definitions stick in the social sciences. Ambiguities arise in other quantities in the fundamental equations in the course of the treatise. Attention has already been called to these difficulties by the critics of the book.[8]

These equations really are but an elaboration of the old income equation of Aftalion, $R = PQ$ or $P = R/Q$ (R being the money income, P the price level, and Q the real income). The only difference is that Keynes defines income to exclude the windfall profits, while Aftalion defines income to include them. Keynes' E, in other words, is equal to Aftalion's R only when the second terms of Keynes' equations are zero.

The advantage of Keynes' procedure is that it enables him to separate out into the second term what he regards as the disequilibrating elements in the price structure. The first term of his equations, E/O, represents the normal long-run tendency of the price level, and it is not liable to sudden changes. It is the normal income of society divided by the actual output. The remuneration of the factors of production is identical with E, so the expression E/O becomes the efficiency earnings of the factors of production, or the rate of earnings per unit of output or the cost of production per unit of output. Prices and costs per unit of output will be equal only at a moment when the economic system is in equilibrium, that is to say, when there are no profits except the normal profits of entrepreneurs, or when there would be no inducement for the entrepreneurs to change output were they free to make new bargains with the factors of production.

[8] The most exhaustive criticisms are made by D. H. Robertson in the *Economic Journal*, September, 1931 and by Hayek, in his articles in *Economica* in August and November 1931, and February 1932.

It is clear, therefore, that E/O is essentially a "normal" conception. Actual income will probably seldom ever equal E exactly. It is only when costs and prices exactly coincide that this equilibrium position is achieved. Whenever costs fail to equal prices the economic system is in disequilibrium. In Keynes' equations this at once shows up in the second terms. S is now no longer the same as I' or I. Windfall profits or losses occur, and the price level rises above or falls below the cost of production.

The equations, therefore, only express a truism, like any other equation of exchange. The virtue of the truisms, which equations of exchange reflect, is entirely dependent on the success with which they throw into relief the causal factors at work. Now this is what Keynes claims for his equations. But the definition of E which he gives necessarily brings it about that the second term can only *reflect* a change in prices, and precludes the second term from displaying the causal factors at work. This is so because, when entrepreneurs experience losses, they do not receive their full normal income. Since savings, S, are defined as the sum of the differences between the normal money incomes (E) of individuals and their actual expenditure on current consumption, the amount by which their actual incomes fall short of their normal incomes is a part of savings (S). Thus entrepreneurs' losses are, by definition, savings. Whenever they experience losses, savings increase, the second term of the equation becomes negative, and prices are lower than costs. But whatever is the cause of disequilibrium in the price-cost structure is surely the cause of this fall in prices. The cause is not an excess of savings. The excess of savings just reflects the fall in prices. The analysis of the causes of the fall should concentrate upon the causes of the disequilibrium of prices and costs. To attempt to accomplish this by analysing the forces behind the decisions of savers to save, and the decisions of entrepreneurs to invest, is to assume that the losses which constitute a part of savings are due to a voluntary decision on the part of the entrepreneurs to save. It is this thatKeynes does, when he insists that his equations usefully display the causal forces at work. To sum it up in the words of Mr. Hawtrey,[9] ". . . a

[9] See *The Art of Central Banking* by R. G. Hawtrey, p. 349. Hawtrey points out that it is not until prices have changed that any disequilibrium occurs in I and S. The first shock of a change in demand is borne by stocks, and prices are not changed. So that while the disequilibrium is already beginning to show itself in large stocks, I and S still balance. On the basis of this he seeks a less rigid interpretation of Keynes by abandoning entirely the fundamental equations, and redefining investment and saving as capital outlay and unspent income. Capital outlay differs from investment in that it does not include the involuntary increases or decreases of stocks above normal. It is a very dynamic concept, and may prove very useful for the analysis of particular questions at

difference between savings and investment cannot be regarded as the cause of a windfall loss or gain, for it *is* the windfall loss or gain."

One thing clearly does result from the enquiry which Keynes undertakes into the motives behind the decisions. This constitutes a considerable clarification to business cycle theory. Keynes succeeds in making clearer than others have done the part played by hoarding in the business cycle. The decision of a saver to save is not enough. It is necessary for him also to decide the form which his savings shall take. Sometimes the decision is to hold the savings in the form of capital goods, or securities; sometimes it is to hold the savings in a more liquid form, that is to say in the form of savings deposits. "The curve, representing the relative preference of capital owners for savings deposits and other capital assets respectively . . . in which the abscissa measures the quantity of hoards held when the price of non-liquid assets in terms of liquid assets (or hoarded money) is given by the ordinate" is what Keynes calls the "propensity to hoard," or the "degree of bearishness." It is as necessary carefully to distinguish this conception from the actual volume of hoards (or savings deposits) as in the theory of value we must distinguish a demand curve from demand in the sense, that it so often has with laymen, of the quantity of goods bought. The quantity of hoards which the public will hold will vary with the price of investment goods. The banking system in Keynes' view is able to control the volume of savings deposits. Adjusting this to the bullishness and bearishness of the public it is able to enter into the situation as a controlling factor.

If the public and the entrepreneurs are anxious to hold their savings or their profits in a liquid form, that is to say, in savings deposits, the banking system is able to nullify this by increasing the volume of these deposits and itself offsetting the bearishness of the public by purchasing the securities which the public is now less ready to hold. Admittedly, in times of deep depression, an expansion of new investment is not easy to bring about. It is for this reason that Keynes regards more direct stimulation of investment as a necessary part of the remedy in such times. But, as a preventive expedient in normal times, the banking system could, in a measure, perform this task.

The precise rôle of the banking system is, however, not entirely clear in Keynes' analysis. In particular, he seems to have failed satisfactorily to account for changes in the *effective* supply of money—MV in contrast to M in Fisher's terminology—upon the relation of the two terms of his

certain stages of the business cycle. We are inclined to question Mr. Hawtrey's analysis, however, because it places stocks far more prominently to the fore than Mr. Keynes would wish; and, moreover, it assumes away what has to be analyzed, namely, the reason for the abnormal increase or decrease of stocks.

equations. He argues that, even in a progressive society, the price level can be stabilized by equalizing saving and investment. But it is not clear how both these ends could be achieved, since the attempt to stabilize investment and saving would tend to hold the effective supply of money constant.

The introduction of new bank money into the economic system must take place through entrepreneurs or through consumers. If the former (and surely this is by far the most important case), the funds advanced are used generally for investment. It thus seems reasonable to suppose that in this case new bank money, except in so far as it is offsetting changes in velocity, will produce investment without saving, or, in other words, will produce disequilibrium in the second term. In Hayek's terminology forced saving will have taken place. If, on the other hand, the new money finds its way into the system through the consumers—and it seems necessary to suppose that Mr. Keynes believes something of this sort[10]—it may be argued that this does not *directly* affect investment. But, as we shall show in the section on Hayek, the indirect effect of such an increase is that producers, spurred on by the enlarged profits which the higher prices of consumer goods will now make possible, will seek to expand their borrowings at the banks, and if they succeed this will lead to disequilibrium of investment and saving. Moreover, an increase in the loans to consumers would raise the value of consumers' goods (PR) and so, from Keynes' definition of savings, would necessarily cause S to decline.

These considerations suggest the relationship of changes in the effective supply of money to windfall profits and losses. In the prosperity phase of the cycle, forced savings are equal to Keynes' windfall profits, in which case we have investment equal to forced saving plus voluntary saving, or in Keynes' terminology $I=Q+S$. For, so long as windfall profits are being made, there is no distinction between Keynes' S and voluntary saving. The windfall profits are the result of forced saving arising from an expansion of the effective money supply. In the depression phase of the cycle, however, when windfall losses are sustained, Keynes' S equals voluntary savings plus Keynes' windfall losses. Windfall losses result from prices falling below costs, and this is occasioned by a contraction in the effective money supply.

In our view, then, changes in the effective money supply necessarily cause disequilibrium between investment and saving in Keynes' sense. In a progressive society, where outpu is continually increasing, E/O, and hence the price level Π, cannot be stabilized without producing disequilibrium in the second term of the equation.

[10] See *Economica*, November 1931.

It is not a sufficient answer to say that the old theories are here inapplicable, and that we have to don a new pair of trousers to understand them.[11] It is essential to show the relationship between the two. It appears that the novelty of the new approach has led Keynes to neglect the discipline of the old. Until the relationship between the two is made clear, it is impossible not to believe that his critics are essentially right and that the very core of his analysis is fallacious.

How then shall we appraise Keynes' work? If we adhere rigidly to Keynes' definitions of investment and saving, we encounter difficulties. If we discard the definitions, as very often Keynes himself seems to do, and interpret investment and saving in accordance with generally accepted terminology, the equations then fall to the ground. Yet, when this is done, we often find suggestive and helpful analysis. The position which Keynes' book seems to us to hold may perhaps be made clear if we compare it to another masterly work. Without making any absolute comparisons, we may liken it in some respects to *Das Kapital* of Karl Marx. Here is a work which ploughed deeply into ground only lightly dug before, which did so inadequately by developing a fundamentally incorrect theoretical structure, and yet withal a work which contained a multitude of correct ideas, greatly obscured by the unsatisfactory nature of the foundation, yet offering a host of illuminating suggestions. Both the truth and the error have been used by ardent protagonists in the political arena to support practical policies in which they believe. The likeness of the *Treatise on Money* to *Das Kapital* in all these respects is striking. Emphatically it is a book which no one can dismiss lightly, and which no seeker after truth can afford to ignore.

The errors in the fundamental analysis, moreover, do not, lead Keynes into so dangerous a program of practical policy, so far as the control of credit is concerned, as that to which Hayek is forced. For, though Keynes reads into his criterion the equalization of saving and investment, he really has in mind, as he clearly states, the stabilization of a price level. In our view, his program in this regard is too rigid, and fails to take due cognizance of the needed flexibility which moderate cyclical price movements provide.[12] Yet at many points he is ready to give full weight to the frictional forces which are encountered in real life. Thus it is that he advances as one of the virtues of the standard he finally adopts, that it would have the effect of gradually reducing the

[11] We are aware that Mr. Keynes will not admit the argument. See his answers to Robertson and Hayek in *Economic Journal*, September 1931, and in *Economica*, November 1931.

[12] See Alvin H. Hansen, *Economic Stabilization in an Unbalanced World* (Harcourt, Brace & Co., 1932), Chapter XX.

obligations arising out of past borrowing. This touch with reality, of which Mr. Keynes frequently gives evidence in the practical application of the equations, animates a large part of the second volume, and helps him to avoid dangers on many occasions.

But there are other directions in which the errors in the fundamental analysis have strewn the path with many pitfalls. This is particularly true when he treats of the artificial stimulation of investment by various governmental devices. It is here that it is necessary, as Hayek suggests, to pay more thorough attention to the tendencies which the equilibrium analysis of static economics makes clear, and to warn against the mechanistic applications of Keynes' formulae to practical policies. His equations are in danger of being used as a kind of slot machine into which one may insert a question and draw out the correct answer. Tested by the second term of the equations, it becomes too simple a matter to judge the value of various remedies for depression. Any line of action which will tend to raise the rate of investment wins support, and, similarly, anything which decreases the volume of saving is looked upon with favor. There is thus danger that absurd conclusions may be arrived at with respect to governmental deficits, tariff policies, buy-now campaigns, unemployment doles, and even public work programs.

There is a great danger in selecting certain formulae that do not form a closed determinate system, and applying them mechanically to the complex phenomena of economic life. In so doing one is apt to overlook the indirect effects of the program under consideration. One might assume that a tub, full to the brim, would weigh still more were a block of ice placed in the water. The total weight would indeed have been greater were it not that the addition of the block displaced an amount of water weighing precisely the same as the block. So it is with the magnitudes of economic phenomena, whose effect on prosperity or depression we seek to assess. It might indeed be true, as Keynes averred, that a billion-dollar treasury deficit[13] in the United States in 1931, by reducing the volume of saving, would have been just as beneficial in its effect upon business recovery as would, for example, the erection of a gigantic power plant which promised profitable returns and, in addition, the prospect of lower costs to manufacturing industry, were it not that the indirect effects of such a deficit, at the same time that it reduced the volume of saving, might easily, through the pessimism engendered thereby, curtail the volume of private investment by an even greater amount. It might well be that a tariff on

[13] See Keynes, in *Unemployment as a World Problem* (University of Chicago Press, 1931), p. 25.

British imports would stimulate the volume of investment in Britain and so tend to revive British industry, were it not that this very action might cut off the possibility of sales by British exporters abroad. In economic matters it is always extremely dangerous to apply formulae mechanically, since the problem is essentially one of a complex equilibrium involving, amongst other things, social psychology. The effect of a certain line of action often cannot be measured, because it is not known beforehand how human beings will react thereto.

Many illustrations could be given of the danger referred to, but it is proposed to consider only one in some detail, namely, the program of public works. This is chosen because it is certainly the most reasonable of all the various schemes which Keynes has advocated as a remedy for the depression, other than central bank policy itself. Moreover, public works are so widely regarded by the public generally as an effective means of securing business revival that they merit the most careful consideration.

The justification of public works must always be something more than that they stimulate investment. We must pay attention to both the indirect effects of their initiation and to the inherent difference, in a private capitalistic economy, between public and private investment. Under certain circumstances, a program of public works definitely operates counter to the beneficial effects which may be expected from a liberal central bank policy. Under other circumstances, such a program, introduced in conjunction with the central bank policy, may prove to be of genuine value if introduced at the right moment. If the capital market is in a strong position and able to absorb issues readily, if funds are plentiful and the credit of the government on a high plane with no question with respect to its ability to balance the budget, then the funds needed for the public works program may be raised without any appreciable burden on the capital market, and therefore without counteracting the easy money policy of the central banks. A policy of public works may, morever, be reconciled with an easy money policy provided the banking system is prepared to absorb the entire public works bond issue and is, in addition, in a position to expand its operations to take care of the demands of private enterprise. If, however, the capital market is weak, if the demand for bonds is highly inelastic, if the credit of the government is uncertain, then a government issue of any considerable size may easily affect the bond market seriously, and thus discourage the expansion of private enterprise and make private flotations almost impossible. Under such circumstances, a program of public works would be very dangerous, and could have no other effect than to prolong and intensify the depression.

It has been argued that a program of public works would not weaken

the bond market, because it would stiffen the demand for commodities, thereby raising the price level, and save private business from distress sales of securities. To this we would reply that the unfavorable effect of a public works program—to be financed by borrowing—is immediate upon the bond market, but that the favorable effect upon the commodity market takes some time to appear. Moreover, the favorable influence is felt only in certain sections of industry, while the unfavorable one is not only earlier, but more general, in its effect.

It is a mistake to suppose that it makes no difference whether the increased rate of investment is undertaken by the government in a program of public works or by private enterprise in an expansion of long-range investment. Whenever the government emits a large issue of bonds it weakens *confidence* in the capital market and therefore postpones, delays, and discourages private investment. If the public bodies were willing to assume a large enough program of public works to fill up the whole gap and restore the rate of investment to a normal level, well and good, but this would mean that the government would have to be willing to undertake enterprises that are far beyond the scope that capitalistic governments have hitherto entertained. So long as we are operating upon the assumption that the fundamental work of production will be carried on by private enterprise, we cannot obtain any sound revival of business until private enterprise enters the investment field.

It appears, therefore, that when it is said, "What difference does it make who is undertaking the investment so long as it is being done?" the remark displays a deep misunderstanding of the difference between public and private investment above outlined. The answer is that the difference it makes to the psychology of private enterprise is enormous. It is the difference between optimism, encouraged by the boldness of some entrepreneurs who have the courage to act on the assumption that profits are in sight, and pessimism, fed by a demoralized capital market and by the prospects of increasing tax burdens in the future.

Again, it may be said that a public works program will stimulate business because it increases the demand for certain raw materials and pays wages to workmen, thus stimulating consumer buying-power. But here again one must not forget that, if the capital market is weak, the moment certain fields are stimulated by public works, a damper may be placed upon private enterprise by a vast issue of public bonds, and so operations elsewhere may be curtailed. The fallacy which lurks here is precisely akin to the familiar tariff fallacy. It is always easy to see the prosperity and activity of the industries protected by the tariff, but what is often overlooked is that at the same stroke the tariff injures and depresses enterprise in other fields. This point is generally

recognized by economists, but strangely enough, many do not seem to have seen that the same argument may apply to the public works question.

We have said enough to show that a public works policy is in danger of getting unwarranted support from a far too mechanistic interpretation of Keynes' equations. Those who advocate a program of this character should do so because they are convinced that unfavorable factors working against its success, such as we have outlined, do not outweigh the favorable factors. Against implicit faith, derived from the fundamental equations, in the artificial stimulation by investment, we cannot warn too strongly.

To sum up, very briefly, we find Keynes' contribution brilliantly suggestive but suffering from the following defects: (1) The first fundamental equation contains a serious error which makes it of uncertain value for any other year than the base year. (2) The equations are of no aid in arriving at an explanation of the causal forces at work. (3) It is impossible to reconcile his two-fold criteria of control, namely: (a) equilibrium in the second term of the equations, and (b) stability of the price level in a progressive society. (4) The equations cannot be applied safely as a guide for public policy.

III

HAYEK'S THEORY

In order to facilitate discussion we propose to state Hayek's theses, as we understand them, in a series of propositions as follows:

Thesis Number 1. That depression is brought about by a shrinkage in the structure of production (i.e., a shortening of the capitalistic process). In Hayek's view, the phenomenon of depression *is* a shrinkage in the structure of production. Dynamic forces may bring about various effects on economic life, but unless they have the specific effect of shortening the process of production, depression will not follow therefrom. Nor does depression ever assume any other form than that of a shrinkage in the structure of production. In short, depression may be defined as a shortening of the capitalistic process of production.

Thesis Number 2. The leading cause (there are, however, others) which brings about, either directly or indirectly, a shortening in the process of production, is the phenomenon of *forced* saving.

Thesis Number 3. An elongation of the process of production caused by *voluntary* saving tends to remain intact; or at least, there is no inherent reason why such an elongation *must* necessarily be followed by a shrinkage in the structure of production.

An increase in voluntary saving would cause an enlarged demand for producers' goods in relation to consumers' goods, and this would raise

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the prices of goods in the higher stages of production in relation to those of the lower stages. The consequent narrowing of the price margins or, in other words, the lower rate of interest, would thus make possible a permanent elongation of the process of production.

Thesis Number 4. A lengthening of the process of production caused by forced saving (the money supply not having been held neutral) cannot possibly be permanently maintained, but must necessarily be followed by a shortening in the process of production.

An increase in money supply (bank credit) made available to entrepreneurs would cause an increase in the demand for producers' goods in relation to consumers' goods, and this would raise the prices of goods of the higher order in relation to those of the lower order. The consequent elongation of the process of production could not, however, be maintained, because a reversal in the price relationship of higher and lower order goods would appear as soon as the money supply ceased to increase owing to the fact that spending and saving habits had not changed. Thus, a shrinkage in the artifically elongated process of production would inevitably occur.

Thesis Number 5. That an increase in consumer demand occasioned by an increase in the supply of money (over and above what may be necessary to hold money neutral) inevitably brings about a shortening in the process of production, and so causes depression.

An increased supply of money made available directly to consumers would cause an increase in the demand for consumers' goods in relation to producers goods, and would thus raise the prices of goods of the lower order in relation to those of the higher order, and this would inevitably bring about a shortening in the process of production.

Thesis Number 6. That excessive public expenditures and taxation, by increasing the ratio of spending to saving, will force a shortening in the process of production and so cause prolonged depression or business stagnation.

An increase in spending would cause an increased demand for consumers' goods in relation to producers' goods, and this would raise the prices of goods in the lower order in relation to those of the higher order. The consequent widening of the price margins between the lower and higher order goods, or, in other words, a higher rate of interest, would, therefore, bring about a shortening of the process of production.

Thesis Number 7. That the supply of money should be kept constant, except for such increases and decreases as may be necessary (1) to offset changes in the velocity of circulation, (2) to counteract such changes in the coefficient of money transactions as are occasioned by the amalgamation of firms, and the like, and (3) to provide for any changes in non-monetary means of payment, such as book credit, that

may be taking place. (A distinction is thus made between a "constant" money supply and a "neutral" money supply.)

Thesis Number 8. That any change in money supply (other than that necessary to hold money neutral) is harmful because it necessarily brings about, eventually, a shortening in the process of production. (a) If the increased money supply goes to entrepreneurs, the process of production is first elongated, but, subsequently, necessarily shortened, returning to its previous status, or to a still shorter process. (b) If the increased money goes first to consumers, the shortening of the process of production takes place at once, and the process remains permanently shortened.

Thesis Number 9. That an increase in production and trade forms no justification for an increase in bank credit.

Thesis Number 10. That a period of depression should not be counteracted by any inflation of the money supply, though, in theory, there is the possibility that during the acute stage of the crisis, while the capitalistic structure is tending to shrink more than will ultimately prove necessary, a nicely regulated increase might prove beneficial. The impossibility of such skillful management makes this an unimportant exception.

* * *

Let it be remembered that these statements are our own formulations of Dr. Hayek's theory. We believe, however, that they present a fair and accurate summary of his position. They are derived from the analysis contained in Chapters II, III, and IV of his *Prices and Production*. They are stated in this formal manner in order to facilitate discussion.

Thesis Number 1 is highly controversial. In a society in which the working population is stationary, a shrinkage in the structure of production would mean not only that the rate of new investment had declined, but that it had fallen to a minus quantity.[14] Suppose new investment fell to zero, but that existing capital was being fully replaced, then the process of production, being neither shortened nor lengthened, would just be maintained. It follows that there could be a violent fall in the rate of investment before any shrinkage in the structure of production occurred.[15]

[14] Dr. Hayek says that "*any* change in the amount of capital per head of working population is equivalent to a change in the average length of the roundabout process of production. . . ." *Economica*, February 1932, p. 42.

[15] See Hayek, *Prices and Production*, p. 44, where he seems to say that the structure of production remains the same if replacement is fully maintained. On p. 45, however, he appears, inconsistently, to say that if the total demand for producers goods whether for replacement or for new capital declines, the process of production is shortened.

In the view of Mr. Keynes, however, if the volume of new investment had fallen to zero, while the rate of saving was above that level, depression would ensue. According to his analysis, depression does not wait upon an actual shortening of the capitalistic process of production. It may occur even though the structure of production is being elongated. Even though there were a considerable volume of net investment, and so an elongation of the capitalistic process, depression would still obtain, provided only the rate of investment fell below the rate of saving. At this point there is, therefore, sharp disagreement between Keynes and Hayek. In the view of Mr. Keynes, general depression is a condition caused by the failure, owing to all sorts of obstructions, of an even flow of the two compartments of income—saving and spending—into the market for producers' goods on the one side and consumers' goods on the other. If this flow is being obstructed by reason of the fact that savings outrun investment, thus causing a considerable amount of purchasing power to be held in abeyance, depression and unemployment would prevail, even though there were some lengthening of the process of production.

Conversely, in Mr. Keynes' view, there might conceivably be a shrinkage in the process of production without any resulting depression. If a minus quantity of net investment were balanced by a minus quantity of saving, no depression, according to Keynes, would ensue.

This view, however, rests on the assumption of the possibility of instantaneous readjustment of production to shifts in demand. With Keynes, a constant totality of demand, regardless of changes in the internal structure of demand, is sufficient to prevent depression. No matter how much the passengers may run around on the deck the ship will not capsize. Only when they all rush madly to one side is the ship's balance disturbed.

But it is just at this point that Keynes fails to take sufficient account of friction as a factor in economic dynamics. Mr. D. H. Robertson holds the far more realistic view that, because of the lack of mobility of factors, any violent shift in production from consumers' goods to producers' goods and back again, or from one important industry to another, causes business fluctuations. "If it be true that industrial development, if we are to have it at all, must needs proceed to a certain extent by jerks, there will be times when a transcontinental railway has just been built which will meet all traffic needs for many years—when the street railway system of the country has been electrified, or its merchant fleet converted to an oil basis, when every man, woman, and child possesses a motor car—in short, when a condition of temporary saturation with some important kind of equipment has been reached, and it is exceedingly difficult for mankind to switch his activities

straight away into the satisfaction of some new need. Under such conditions, and we should be rash to say we would rather they never occurred, the symptoms of industrial depression will develop."[16]

Maladjustment in production, arising from a violent decline in the demand for the products of important industries, will result in unemployment of factors and reduced output. The flow of money purchasing power is obstructed, and time must elapse before the bank credit released from the declining industries can be got out to producers in other industries in sufficient volume to offset the contraction in those branches which are retrogressing. There will follow eventually an increase in prices in other lines, and this, in turn, will finally lead to the employment of the factors displaced by the declining industries. But because of the immobility of labor, and of the other factors, especially when re-employment involves migration to other regions, this takes time, and, in the meantime, production and employment are reduced. If a shift of this character affects industries employing many workers, and if these, in turn, furnish a market for significant related industries, a very considerable depression may grow out of shifts in demand which requires a re-adjustment in the productive forces before a new equilibrium is reached. Depressions of this sort arise not out of a shortening in the process of production, but out of the immobility of the factors of production and the lapse of time required to induce entrepreneurs to utilize the bank credit released by the declining industries.

A special example of the type of depressional factors here under consideration was the closing down of the Ford factories during a large part of 1927 while preparations were being made to produce a new model. The ramifications of this shut-down upon related industries was of a sufficiently large order of magnitude to produce a considerable depression of trade. There was no shrinkage in the structure of production. The decline in employment, production, and purchasing power, was due altogether to the inability to adjust instantly the processes of production to the changed situation.

We think it is clear that the view that the essence of depression is a shrinkage in the structure of production is far too narrow. Indeed, it may be questioned whether the process of production is ever shortened, except in depressions of such great severity as that through which the world is now passing. In minor depressions the net investment of capital may continue, though, to be sure, at a reduced rate. The capitalistic process may not unlikely be lengthened. Only in excep-

[16] D. H. Robertson, *Money* (1929), p. 189. See also Arthur Spiethoff, "Vorbemerkungen zu einer Theorie der Über-produktion," *Jahrbuch für Gesetzgebung Verwaltung und Volkswirtschaft* (1902), pp. 730–33; also his "Krisen," *Handwörterbuch der Staatswissenschaften* (1925), VI, p. 74.

tionally severe depressions will there certainly be a failure to maintain the existing supply of instrumental goods. It is true that "self help" movements among the unemployed result in more direct processes of production; that governments often discard machinery and resort to hand methods in order to furnish more employment; and that low wages, together with an unfavorable capital market, may in some industries result in a relatively larger use of labor and a relatively smaller use of capital. On the other hand, many industries utilize the depression period to reorganize plants and equipment in preparation for increased production at the lowest possible unit cost. It is well known that such technical innovations are going forward even under the difficult conditions confronting industry in this depression. Improvements of this character tend in the direction of a more capitalistic process of production. What the net effect is of these various developments upon the structure of production of the whole economy, is not certain. It is probable, however, that in major depressions the depreciation of capital so far fails of replacement that the whole process of production becomes more direct, less capitalistic, than in the preceding boom.

At all events, it is by no means evident that any shrinkage of the structure of production occurs in minor depressions. In deeper depressions, the rôle played by such shortening of the productive process may become a significant aspect of the recession. But it is the decline in the production of capital goods as a whole, whether new capital or replacement, which is the most outstanding characteristic of a depression period. The mere *decline* in such production does not imply, of necessity, a shrinkage in the structure of production.[17]

With respect to Thesis Number 2, we may at once so far agree as to concede that forced saving is, as banks now operate, a major disturbing force causing business depressions. We deny, however, as will be seen later, that forced saving *as such* is necessarily a disturbing factor. Dr. Hayek would be much nearer the truth if he had pointed to the effect of forced saving upon *fluctuations* in the volume of capital investment. Over-stimulation in the production of capital goods finally brings collapse. But a steady rate of new investment, even when persistently stimulated by forced saving, need occasion no concern. Moreover, it should again be noted that a shrinkage in the structure of production is not synonymous with depression.

No objection can be raised, we believe, to Thesis Number 3 formally

[17] It is of interest in this connection to note Cassel's statement that a "period of advance is one of special increase in the production of fixed capital; a period of a decline, or a depression, is one in which this production falls below the point it had reached." *The Theory of Social Economy* (1923), p. 521.

interpreted. It is correct so far as it goes. It contains, however, an implication with which we cannot agree. It suggests that if the process of production is first greatly elongated and subsequently maintained at the new level, no disturbance to economic equilibrium will follow. To make it concrete, the implication is that if an enormous capital equipment is quickly built up in the automobile industry, the cessation of further net addition to equipment would cause no disequilibrium provided only the achieved process were maintained. The transition, however, from a large program of new investment to mere replacement would entail such an enormous decline in investment that a curtailment of purchasing power and depression would, at least in the absence of counter stabilizing forces, almost certainly ensue.

We come now to a consideration of Thesis Number 4, a very important one. The conclusion here stated we think is in part unsound. Dr. Hayek has here fallen into essentially the same error which he has so roundly criticized in Foster and Catchings. These writers, as is well known, maintained that depression is an inevitable concomitant of the phenomenon of saving. Hayek now announces that forced saving must necessarily result in depression. Both conclusions are invalid. It is true that economic stability might be expected to be far more secure were it not for the continued accumulation of capital. And it is still more true that if forced saving could once and for all be eliminated, economic instability might be expected to be very considerably minimized. But neither voluntary saving nor yet forced saving need of necessity disrupt the economic equilibrium. Rather it is the *fluctuations* in the rate of capital accumulations, whether occasioned by voluntary or forced sav ing, which cause disturbance to the even flow of industrial life. A uniform accumulation of capital, as Bickerdike and others have shown, is not inconsistent with economic stability.

A *steady* and persistent increase in bank credit based on a slowly expanding stock of gold reserves is certainly not unthinkable, though admittedly, in the present state of central bank management, not easily achieved. At any rate the question is not here whether or not a stable growth of bank credit is possible of practical attainment. The question is whether any increase in bank credit, supplying funds for investment which had not previously been saved, would of necessity cause disequilibrium. In our view there is no reason whatever to assume that such would be the case.

Dr. Hayek's thesis, and indeed that of the entire Vienna school, is, at this point, not wholly tenable. A part of their argument is quite correct, namely: that, under the artificial stimulus of forced saving, capital accumulation is developed to a point which is in excess of that which can permanently be maintained on the basis of voluntary sav-

ing. As soon as the artificial stimulus is removed, therefore, it is discovered that the structure of production has been elongated to a point which requires more capital than can be maintained by voluntary saving. Thus far the reasoning is sound.

It is next argued that a contraction in the capitalistic structure must necessarily sooner or later follow from every elongation of the process which is brought about through forced saving. This conclusion, however, does not of necessity follow. It rests on the assumption that the increase in bank credit cannot continue indefinitely, and that therefore reliance must sooner or later be placed wholly upon voluntary saving. This is invariably the reason assigned by Hayek, in *Prices and Production*, for the alleged inevitable crisis. But it is just this point that is wrong. There is no reason whatever why the volume of bank credit might not *steadily* continue to rise indefinitely provided either that sufficient gold reserves were forthcoming, or that bank efficiency in the use of gold progressively improved. Or, if a managed currency were substituted for gold, a slow and persistent increase in bank credit might continue indefinitely.

A moderate degree of forced saving *as such* is not necessarily an evil. A shibboleth has of late been made of this term, so much so that, with some economists, the belief in its evil effects has become a fixed superstition. The persistent steady pressure of forced saving would be beneficial to society. What is disturbing is *fluctuations* in the quantity of bank credit, causing now a large volume of forced savings, and now complete cessation thereof. It is not forced saving as such, but its *irregularity* under existing bank practices which causes disturbance. But that is quite a different matter from the argument presented by Dr. Hayek.

No more stable period in the economic history of the last hundred years can be found than that of 1896 to 1914, a period of continued increase in bank credit and slowly rising prices. There were *fluctuations* in the volume of bank credit which were disturbing, but less so than in periods of limited bank credit and decline in prices. The forced saving of this period stimulated investment, but not an excessive amount of instability. Moreover, the non-monetary causes of instability operating in this period should not be overlooked.

Thesis Number 5 is, we believe, not tenable. There is assumed an expansion of consumer demand arising from an issue of money directly to consumers. This, it is claimed, will inevitably result in a shrinkage in the structure of production.

It will be noted that it is assumed that the supply of money and bank credit is elastic. Under these conditions Hayek's conclusion is invalid. If the prices of consumers' goods rise in consequence of the

payment of more money to consumers, the *prospective* rate of profit on output in all the various stages of production will rise. Entrepreneurs will apply to the banks for credit in order to take advantage of the promising prospects. The expanding volume of producer credit will forestall the shift of non-specific goods from the higher stages to the lower stages of production, as Hayek has it. The effort of consumers, in consequence of their increased money incomes, to shift production from goods of the higher order to goods of the lower order will be frustrated by the producers who, tempted by the prospective rate of profit, will at once arm themselves with more money purchasing power.

Hayek's mistake at this point arises from the failure to distinguish between a real change in the demand for consumers' goods occasioned by a fundamental modification in time preference for present and future goods, and a nominal change in demand arising from an increase in the volume of money. A rise in the rate of interest, following from increased spending habits, would bring about a rise in the prices of consumers' goods in relation to producers' goods. Under the assumption of a fixed money income, consumers' goods would then rise in price and producers' goods would fall. But a rise in the prices of consumers' goods occasioned by putting more money into consumers' hands will not cause producers' goods prices to fall, for entrepreneurs, enticed by the wider margin of profit in all the lower stages of production, will anticipate higher profits in all the various stages and will therefore apply for bank credit and seek to expand operations.[18] Indeed it is likely that producers' goods prices will rise much higher than consumers' goods prices for the well-known reason, assigned by T. N. Carver, Albert Aftalion, and J. M. Clark, that a slight rise in consumers' goods prices greatly widens the margin of profit which, capitalized, causes a large increase in the prices of producers' goods.

We must distinguish two quite distinct influences operating on the prices of consumers' and producers' goods: (1) changes in the rate of time preference; (2) expansion of the money supply. The latter may, indeed, indirectly affect the former somewhat, owing to the effect of the shift in the distribution of income incident to a rise in money income and prices. But to clarify our thinking we must hold the two quite distinct in order to isolate the effect of each upon the relative prices of consumers' and producers' goods.

Assuming a fixed money supply, a rise in the rate of interest, consequent upon a change in the time preference of income receivers, will of

[18] If we assume all the factors employed, as Hayek does, this would simply mean that entrepreneurs would bid up the prices of the factors without succeeding in enlarging output.

necessity cause consumers' goods prices to rise and producers' goods prices to fall. This increase in the price margin between goods of the higher order and goods of the lower order is, as Hayek rightly points out, the very essence of an increase in the rate of interest. The demand for consumers' goods is increased in relation to the demand for producers' goods, and the prices of the former rise in relation to the latter.

This is not, however, what Carver, Aftalion, and Clark are thinking of when they say that a slight rise in the prices of consumers' goods will bring about a greater rise in the prices of producers' goods. They are thinking, not of the effect of a change in the ratio of spending to saving on consumers' and producers' goods prices, but of the effect of an increase in money supply upon these prices. An increase in money supply will raise the prices of both consumers' and producers' goods, and it is likely to raise the prices of producers' goods more than those of consumers' goods.

It is the lag of costs behind the rise in selling prices, incident to an expansion of money and credit, together with the lag of the market rate of interest behind the rise in the prospective rate of profit, which causes the prices of producers' goods to rise more than the prices of consumers' goods. The higher rate of profit capitalized at a lagging market rate of interest raises the prices of producers' goods. Producers' goods prices will remain out of line with consumers' goods prices so long as costs remain out of line with selling prices and so long as the market rate of interest lags behind the rate of profits. Eventually this disequilibrium in the internal price structure will be rectified, whereupon the prices of producers' goods will reach a new equilibrium in relation to the prices of consumers' goods.

It makes little difference upon the relative prices of producers' and consumers' goods whether the increased money supply flows directly to consumers or indirectly to consumers by way of entrepreneurial borrowing. Entrepreneurs will borrow more at the banks only if and when the prospect of profitable investment arises. This can only arise in consequence of some event which has affected, or appears likely soon to affect, the profit margin. And the profit margin can only be changed by either a lowering of unit costs or an increase in sales prices. Whichever happens, business men will wish to borrow more money. If the initial cause of the widening of the profit margin is an expansion of consumer credit, entrepreneurial credit will also quickly be expanded. If the initial cause of a wider profit margin is lower costs, entrepreneurial credit will expand, but this will quickly mean an increased supply of money to consumers.

The conclusion is that the prices of consumers' and of producers' goods move in opposite directions if we assume, first, a rise in the rate of

interest, incident to a shift in the rates of spending and saving, and, second, a fixed money supply. On the other hand, the prices of consumers' and of producers' goods will move in the same direction if we assume an increase or decrease in the money supply.[19] In the latter case the prices of producers' goods will likely fluctuate more violently than those of consumers' goods, but when equilibrium is finally established, their relation to each other will be found to be unaltered unless the effects of the transition period have been such as to change permanently the time preference of income receivers. This is not unlikely, since an increase or decrease in the money supply is likely to increase or decrease the real income of the community.[20]

Some qualifications are, however, necessary. If the treasury of the United States should begin to hand out money in large volume to consumers, it is of course likely that confidence would be impaired, gold would be hoarded, securities would be unloaded, and the prices of producers' goods would actually fall. But this is quite a different matter. This action would threaten the maintenance of the gold standard. But such difficulties arise from the fundamental character of modern monetary systems. If we had a managed paper currency in which the public had, through long experience, acquired confidence, no such results would follow from the issue of money to consumers. Dr. Hayek is not discussing the effect of such issue upon the operation of the gold standard; what is under discussion is whether or not there exists an inherent connection between the issue of credit to consumers and the effect of such issue upon the structure of production in any sort of money system whatever. And on these grounds his conclusion will not, we believe, stand the test of close analysis.

With respect to Thesis Number 6 we readily agree that it is wholly fallacious to assume that a government can, in a private capitalist economy, spend its way out of depression. Public expenditures, by

[19] This analysis, we believe, also helps to reconcile the positions of Keynes and Robertson with respect to this question: (See *Economic Journal*, September 1931). Unless there is a change in the *effective* quantity of money—and this involves not only the quantity of bank deposits, but also the propensity of the public to hoard—a rise in the prices of consumers' goods must bring about a fall in the prices of producers' goods.

[20] A curious but absurd conclusion follows from Hayek's analysis if we invert his assumptions. Suppose, instead of increasing consumers' credit, it is contracted. Following Hayek's reasoning, a decrease in the supply of money paid to consumers should raise the prices of goods of the higher order in relation to goods of the lower order, and thus cause a movement of non-specific goods from the lower stages to the higher stages of production. This would mean an elongation of the structure of production and so, according to Hayek, a spurt of prosperity.

placing a heavy burden upon either the capital or money markets, or both, may retard the process of recovery of private enterprise. Moreover, it is our view that any *sudden* increase in taxation might easily result in a serious decline in the rate of private investment and so cause depression. But here again we must be careful to distinguish between: (1) the proportion of the national income taken in taxes, as such; and (2) violent changes in this proportion. Sudden increases in the tax burden are surely disturbing. But it does not follow that a larger amount of community spending at the expense of private spending, or even at the expense of capital accumulation would cause economic instability. Once equilibrium had been reached on the new level of public expenditures, there is no reason whatever to suppose that the proportion of such expenditures to total income is of itself a cause of concern for those interested in stability. It is not the absolute volume of saving or spending but violent *changes* in the ratio of saving to spending which is significant.

The Theses numbered 7, 8, 9 and 10, may best be treated together in order to avoid needless duplication. In our view none of them is valid.

The whole problem as to which is the most desirable: (1) a constant money supply, (2) a money supply expanding with the volume of trade, or (3) a money supply expanding a little faster than the volume of trade, simmers down to the question of frictions. Which of these conditions is likely to facilitate the speediest adjustments to such dynamic changes as increased productivity growing out of improved technical processes? This is the significant question.

The argument for a constant, or more accurately speaking a "neutral," money supply must rest upon the assumption that friction would be minimized under this plan. In our view this argument is based upon a misconception of the functioning of modern economic institutions. It presupposes, in the first place, that prices are, in point of fact, reduced *at once* wherever technical improvements lower cost. Under the modern development of cartels, and other monopolistic or quasi-monopolistic organizations, this assumption does not correspond to reality. An important result follows from the failure of individual prices to adjust themselves at once to lower costs; namely, a violent shift in the distribution of the nation's income, profits absorbing a larger share and wages a smaller, owing to the displacement of workers incident to the combined effect of technical improvements and the maintenance of the sales price. This frequently results in an abnormal increase in reinvested capital and thus creates an unstable situation.

Moreover, it must be remembered that a neutral money policy is of necessity a central bank policy. It is not possible for the central bank

to lower the individual prices of those commodities which, as a result of technical progress, are produced at lower unit costs. All the banking system can do is to keep money constant (or, more accurately speaking, neutral). The price *level* will, in consequence, fall, but there is no assurance that the decline will take place in the commodities produced at lower costs. If the prices of these commodities are held above cost the effect of a neutral money policy will be to force the prices of commodities down whose costs have not fallen at all. It will thus be seen that at the crucial point in his theory, Hayek's emphasis upon *individual prices* is of no avail.[21] His neutral money policy cannot take account of individual prices in the manner necessary to preserve equilibrium in the internal price structure; it can only ensure that prices somewhere (but not necessarily the right ones) shall fall sufficiently to offset the increased output of goods. In short, a neutral money policy would be no guarantee that prices would be properly adjusted to costs. On the contrary, such a policy would almost certainly cause maladjustments in the price structure, since in many industries it would force prices below costs.

How disastrous a neutral money policy might be can be clearly seen from the history of prices in the United States from 1873 to 1896. During this period wholesale commodity prices[22] fell 2.9 per cent per annum. This is not widely different from the decline in prices which a neutral money policy would impose upon a society making as rapid technical progress as was made in these decades. Yet this period witnessed depressions of extraordinary severity and duration. We do not contend that the fall in prices *per se* was an evil. Could it have taken place without causing internal price maladjustments all would have been well. But this, in a world of frictions, was not possible.

It has been suggested that the serious collapse of industry in the United States during the last three years was caused, in large part, by the fact that the price level for some years prior to 1929 was substantially stationary, and did not fall as rapidly as technical progress warranted. In view of the many periods in our history of mild price increases, which were followed by only minor depressions, this reasoning

[21] To this Hayek might reply that for perfect equilibrium the elimination of quasi-monopoly is necessary. With this we would cordially agree, but the postulate is inconsistent with the facts. In spite of this, Hayek is prepared to utilize his theory as a guide to central bank policy. When he does this he has no right to assume away frictions. Under assumptions of complete flexibility we should, of course, be prepared to agree that a neutral money policy would not have these effects.

[22] We have used the variable weight index of Professors Warren and Pearson of Cornell University.

appears to be of doubtful validity. Moreover, the facts are not quite as stated. From 1926 to 1929 the wholesale price index of all commodities[23] declined 1.6 per cent per annum. If we measured the decline from 1925 it would be even greater. Moreover, the prices of non-agricultural products—the ones most involved in Hayek's argument—fell by 2.9 per cent per annum. As is well known the late boom was the only one in half a century in which commodity prices actually fell materially. On Hayek's reasoning the ensuing depression should have been one of the mildest in our entire history.[24]

Admittedly, prices in many industries were too high. But there were also many industries in which they were too low. It is by no means certain that this maladjustment in relative prices would have been reduced had banking policy forced a sharper reduction in the general price level.

Nor does Hayek's thesis take cognizance of the effect of two modern institutions both affecting wage rates, namely: (1) piece rates, and (2) trade unions. Dr. Hayek's thesis requires that the money earnings of the factors of production should remain constant while individual prices fall in line with lower costs. In point of fact, under modern conditions there is a tendency for the earnings of labor to rise. There is, in the first place, the constant pressure of trade unions, and even where this is of relatively little importance, as in the United States, the widespread application of piece rates tends to force earnings up. Even though it be granted that the piece rates are generally set on a new basis whenever a new technical process is introduced, it is nevertheless a fact that following the introduction of an innovation money earnings do rise. Moreover the constant incentive to higher personal efficiency is a persistent force tending to raise the money earnings of piece workers. If disequilibrium were to be avoided, under Hayek's neutral money, piece rates would constantly have to be lowered, and every pressure of trade unions for higher wages would have to be sternly resisted. That frictions, maladjustment, and disequilibrium, would develop under these conditions is not difficult to foresee.

It makes no great difference whether the price level rises slightly, falls slightly, or remains stationary. What is important is that prices shall not change so rapidly, whether up or down, that the cost and

[23] See new revised index of Bureau of Labor Statistics.

[24] There is no clear evidence yet that technical progress, if we take all branches of industry and trade into consideration, made any more rapid progress in the United States during the post-war decade than that experienced before the war. See the studies of Professors Warren and Pearson of Cornell University, on per capita output, and also those of Dr. Carl Snyder of the New York Federal Reserve Bank.

price relationship is thrown out of equilibrium. It is of utmost importance that windfall profits and windfall losses should be avoided, and this requires that costs and prices shall be kept in line with each other. Disequilibrium results from a disruption of the internal price structure. It is idle to make a fetish of a stable price level or yet of a falling one. None of these is of any significance *per se*. A sharp change in prices, whether up or down, brings about a redistribution of income owing to the failure of costs to adjust themselves quickly to price changes. A sharp fall in prices raises costs above prices and results in losses; a sharp rise in prices lifts prices above costs and results in abnormal profits. It is in the forces which produce changes of this character that we must look for the causes of business booms and depressions.

Whether we should favor a gently rising, a gently falling, or a stable price level will depend, in our opinion, upon the view taken of the following question: What sort of price level will, under the impact of dynamic forces, most easily and quickly facilitate the adjustment of costs to prices or prices to costs, as the case may be, so that abnormal profits or losses may be avoided. This is the essence of economic stability. Considering the character of the frictions engendered by modern institutions, it is our view that, on the whole, costs and prices could be best adjusted by a gently rising price level. But we are not obsessed with this conclusion. A gently falling price level might, as Alfred Marshall stated before the Gold and Silver Commission, prove to be quite consistent with a satisfactory equilibrium between costs and prices.[25]

UNIVERSITY OF MINNESOTA.

[25] A special point with respect to Thesis Number 10. Hayek's view is that depression should not be counteracted by any inflation of the money supply.

Let us consider this thesis in the light of the present situation in the United States. Roughly speaking both the velocity of circulation of demand deposits and industrial production have been cut in two. The price level has fallen approximately in the same proportion as the volume of bank credit has contracted.

With recovery, the physical value of trade will of course expand. Should not the money supply—whether in terms of velocity (V) or volume of credit (M)—be permitted to rise? Were this not permitted prices would fall still lower with every increase in the volume of trade. Such a situation would tend to block every effort at recovery. An expansion of credit, either in terms of velocity of circulation or quantity outstanding is a *sine qua non* of business revival.

ON INTERPOLATION

By J. Shohat (Jacques Chokhate)

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The object of this paper is to review certain polynomial and trigonometric interpolation formulae. While the material herein contained is, in general, not new, its aim is to emphasize certain properties of known interpolation formulae, important in practical applications. We confine ourselves to real functions in one variable. For the sake of brevity, the proofs involved are generally omitted. The reader is referred for details, by means of numbers in [ ], to a short bibliography at the end of this article.

1. *General method of interpolation. Convergence. Stability. A sufficient condition for stability.*—The general method of interpolation can be described as follows.

Given a function $f(x)$ of a certain class $[f]$ of functions, single-valued and finite in a certain interval (a,b)—finite or infinite. Choose a set of points $\{x_{i,n}\}$ in (a,b) and a sequence of functions $\{\phi_{i,n}(x)\}$ $(n=1,2, \cdots; i=1, 2, \cdots, n)$ of a certain class $[\phi]$, also single-valued and finite in (a,b), and form the expression

$$\Phi_n(x;f) = \sum_{i=1}^{n} \phi_{i,n}(x) f(x_{i,n}). \tag{1}$$

The functions $\{\phi_{i,n}(x)\}$ are so chosen that

$$\phi_{i,n}(x_j) = \begin{cases} 0, & j \neq i \\ 1, & j = i \end{cases} \qquad (i, j = 1, 2, \cdots, n). \tag{2}$$

It follows that $\Phi_n(x;f)$ in (1) has the fundamental property

$$\Phi_n(x_{i,n};f) = f(x_{i,n}) \qquad (i = 1, 2, \cdots, n). \tag{3}$$

In view of (3), we take $\Phi_n(x;f)$ as an approximate representation of $f(x)$ in (a,b). We designate $\Phi_n(x;f)$ as *the interpolation formula for* $f(x)$, corresponding to a given choice of $\{x_{i,n}\}$ and $\{\phi_{i,n}(x)\}$ and to a given positive integer n.

There are many possible generalizations of (1). We may, for example, consider functions in more that one variable; or, when dealing with differentiable functions, we may require, in addition to (3), that $\Phi_n^{(k_i)}(x_{i,n};f) = f^{(k_i)}(x_{i,n})$, where k_i are given positive integers. All such generalizations will be left out of the discussion which follows.

We write

$$f(x) = \Phi_n(x; f) + R_n(x; f), \tag{4}$$

where the remainder $R_n(x; f)$ has the property

$$R_n(x_{i,n}; f) = 0 \qquad (i = 1, 2, \cdots, n). \tag{5}$$

The interpolation formula (1) gives rise to two important questions.

C. *Convergence.* By this we mean the following property: *is lim* $\Phi_n(x; f) = f(x)$, *convergence uniform over the whole interval* (a,b) *or a part of it, as* n*—the number of points* $x_{i,n}$ *where* $\Phi_n(x; f)$ *coincides in value with* $f(x)$*—increases indefinitely?* In other words, under what conditions can we state that

$$\lim_{n\to\infty} R_n(x; f) = 0 \text{ uniformly in } (c, d)(a \leqq c, d \leqq b), \tag{6}$$

$f(x)$ being any member of the class $[f]$ or of a certain subclass thereof?

This question is highly important in theoretical investigations, for its answer in the affirmative carries the possibility—often very advantageous—of replacing $f(x)$ by $\Phi_n(x; f)$, with n taken sufficiently large.

In practical, especially in statistical, applications, n is necessarily finite; moreover, the functional values $f(x_{i,n})$ used in (1) are affected by certain errors $\epsilon_{i,n}(\gtrless 0)$, so that in (1) $f(x_{i,n})$ and $\Phi_n(x; f)$ should be replaced respectively by $f(x_{i,n})+\epsilon_{i,n}$ and $\Phi_n(x; f)+\Delta\Phi_n$, where

$$\Delta\Phi_n = \sum_{i=1}^{n} \phi_{i,n}(x)\epsilon_{i,n}. \tag{7}$$

Hence, the following question is of extreme practical importance.

S. *Stability. How much is the interpolation formula* $\Phi_n(x; f)$ *in* (1) *affected by errors in the functional values* $f(x_{i,n})$ *used in its construction?* In other words, if in (1), one or several of the functional values $f(x_{i,n})$ are known with errors not exceeding a certain small positive quantity ϵ, will the corresponding error $|\Delta\Phi_n|$ in $\Phi_n(x; f)$ also not exceed ϵ, for all x in (a,b) and for at least sufficiently large values of n? In symbols, is

$$|\Delta\Phi_n| \leqq \epsilon, \text{ for } a \leqq x \leqq b \text{ and } n > N, \text{ if all } |\epsilon_{i,n}| \leqq \epsilon? \tag{8}$$

Or, on the contrary, in place of (8), in spite of the smallness of ϵ, does $|\Delta\Phi_n|$ become very large, as n increases, for at least some values of x under consideration?

In the first case, when (8) holds true, we call the interpolation formula (1) "*stable,*" while in the second case (1) is said to be "*unstable.*"

The notion of stability, as defined above, refers to possible errors in the functional values used of the function $f(x)$ to be interpolated.

(7) shows that $\Delta\Phi_n$ *does not depend on the nature of* $f(x)$. Hence, *stability holds for all members of the class* $[f]$ *under consideration, or else it fails for any of them,* while convergence may hold for some members of $[f]$ and fail for the rest. We see at once, that (8) is satisfied, if

$$\sum_{i=1}^{n} |\phi_{i,n}(x)| \leqq 1 (a \leqq x \leqq b; n > N). \tag{9}$$

The inequality (9) *is,* therefore, *a sufficient condition for stability of an interpolation formula.*

2. *Polynomial and trigonometric interpolation of continuous functions.* Hereafter we consider the most important case, where the class $[f]$ consists of all functions $f(x)$ continuous in (a,b) and the class $[\phi]$—of all *polynomials* of degree $n=0, 1, \cdots,$

$$G_n(x) = g_0 + g_1 x + \cdots + g_n x^n,^1$$

or of all *trigonometric sums* of order $n=0, 1, \cdots,$

$$T_n(x) = t_0 + t_1 \cos x + t_2 \sin x + \cdots + t_{2n-1} \cos nx + t_{2n} \sin nx.$$

It follows that $\phi_{i,n}(x)$ in (1) are polynomials or trigonometric sums respectively. We speak then of (1) as of a *polynomial or trigonometric interpolation formula* for $f(x)$. Our discussion deals chiefly with the former.

3. *A necessary condition for convergence*[2] *of a polynomial or trigonometric interpolation formula.*—It can be stated as follows. *The points* $\{x_{i,n}\}$ *must be everywhere dense in* (a,b), *i.e. any subinterval* $(\alpha, \beta) \subset (a,b)$ *must contain some of the points* $x_{i,n}$ *for* n *sufficiently large:* $n > N$.

In order to prove this statement, we make the contrary assumption: there exists a subinterval (α, β) free of points $x_{i,n}$ for $n > N$. Construct a continuous function $f(x)$ as follows: $f(x) \equiv 0$ for $a \leqq x \leqq \alpha_1$, $\beta_1 \leqq x \leqq b$ $(\alpha < \alpha_1 < \beta_1 < \beta)$, $f(x) \equiv 1$ for $\alpha_1 \leqq x \leqq \beta_1$, $f(x)$ rises as a linear function (straight line) from 0 to 1 and falls in a similar manner from 1 to 0, as x varies respectively from α to α_1 and from β_1 to β. For such $f(x)$ the convergence property C leads to

$$1 = f(x) = \lim_{n \to \infty} \Phi_n(x; f) \quad (\alpha_1 \leqq x \leqq \beta_1),$$

which is impossible, for, by hypothesis, $x_{i,n}$ is outside (α_1, β_1), $f(x_{i,n}) = 0$ $(n > N; i = 1, 2, \cdots, n)$ and hence,

$$\Phi_n(x; f) \equiv 0 \quad (n > N).$$

[1] $G_s(x)$ stands generally for an *arbitrary* polynomial of degree $\leqq s$.

[2] The expressions "convergence, convergence property C," when used without further specification, pertain to the whole interval (a,b) and to the whole class of continuous functions.

4. *Lagrange interpolation formula* (=*LIP*).—This is the best known and most frequently used among the polynomial interpolation formulae:

$$\Phi_n(x;f) = \sum_{i=1}^{n} \frac{\phi(x)}{(x - x_{i,n})\phi'(x_{i,n})} f(x_{i,n}) \equiv \sum_{i=1}^{n} \phi_{i,n}(x) f(x_{i,n})$$

$$\phi(x) = \prod_{i=1}^{n} (x - x_{i,n}); \quad \phi_{i,n}(x) = \frac{\phi(x)}{(x - x_{i,n})\phi'(x_{i,n})}. \tag{10}$$

Since a polynomial of degree $n-1$ is uniquely determined by the values it takes at n arbitrarily chosen points, it follows that

$$G_{n-1}(x) = \sum_{i=1}^{n} \phi_{i,n}(x) G_{n-1}(x_{i,n})$$

$$G_{n-1}(x) \equiv \Phi_n(x; G_{n-1}); \; R_n(x; G_{n-1}) \equiv 0. \tag{11}$$

In particular,

$$1 \equiv \sum_{i=1}^{n} \phi_{i,n}(x), \tag{12}$$

and this leads to the following expression for $R_n(x;f)$ in (4):

$$R_n(x;f) \equiv f(x) - \Phi_n(x;f) = \sum_{i=1}^{n} \phi_{i,n}(x)\,[f(x) - f(x_{i,n})]. \tag{13}$$

Rewriting (11) as

$$0 = G_{n-1}(x) - \sum_{i=1}^{n} \phi_{i,n}(x) G_{n-1}(x_{i,n})$$

and subtracting this from (13), we get:

$$R_n(x;f) = f(x) - G_{n-1}(x) - \sum_{i=1}^{n} \phi_{i,n}(x)\,[f(x_{i,n}) - G_{n-1}(x_{i,n})] \tag{14}$$

—an important formula, for it involves an *arbitrary* polynomial $G_{n-1}(x)$, the choice of which is at our disposal.

In order to make this choice the most profitable one, we recall

(a) *Weierstrass Approximation Theorem.*—Any function $f(x)$, continuous in the finite interval (a,b), can be approximated therein indefinitely and uniformly by means of polynomials (of ever increasing degrees).

(b) *Tchebycheff Theorem on Best Approximation.*—$f(x)$ having the above properties, there exists among all polynomials $G_n(x)$ one (and only one)—$M_n(x)$—of "best approximation" to $f(x)$ on (a,b), i.e.

$$(15)\qquad E_n(f) = \max\left|f(x) - M_n(x)\right| \leqq \max\left|f(x) - G_n(x)\right| \text{ on } (a, b).$$

We call $E_n(f)$ the "best approximation" of $f(x)$ on (a,b) by means of polynomials of degree $\leqq n$. The Weierstrass Theorem tells us that

$$(16)\qquad \lim_{n\to\infty} E_n(f) = 0.$$

We now consider *LIP* (10) for a *finite interval* (a,b). If we take in (14) $G_{n-1}(x) \equiv M_{n-1}(x)$, we get:

$$(17)\qquad \left|R_n(x;f)\right| \leqq E_{n-1}(f)\left\{1 + \sum_{i=1}^{n}\left|\phi_{i,n}(x)\right|\right\} \quad (a \leqq x \leqq b).$$

Thus the behavior of $R_n(x;f)$, for n very large, depends on that of $F_n(x) = \sum_{i=1}^{n}\left|\phi_{i,n}(x)\right|$. It has been shown by Faber [13] that the necessary and sufficient condition for *LIP* to possess the convergence property C is

$$\lim_{n\to\infty} F_n \neq \infty \quad (F_n = \max F_n(x) \text{ in } (a, b)),$$

but that

$$(18)\qquad \lim_{n\to\infty} F_n = \infty,$$

for any choice of $\{x_{i,n}\}$. Furthermore, the smallest possible order of magnitude, with respect to n, of F_n is $\log n$, and this order is actually attained [8] (if $(a,b) \equiv (-1,1)$, for $\phi(x) = \text{Const.} \times \cos n \text{ arc } \cos x$). We thus conclude, with Bernstein and Faber, that *LIP does not possess the convergence property C, no matter how we choose the points* $\{x_{i,n}\}$. Our second conclusion is the following: it is possible to construct an *LIP* convergent uniformly in (a,b) for the subclass of continuous functions satisfying the Dini-Lipschitz condition

$$(19)\qquad \left|f(x+\delta) - f(x)\right| \cdot \left|\log\left|\delta\right|\right| \to 0, \text{ as } \delta \to 0 \ (a \leqq x, x + \delta \leqq b).$$

In fact, (19) implies, as is known,

$$\lim_{n\to\infty} E_n(f) \log n = 0,$$

and if $\phi(x)$ is so chosen that F_n is of order $\log n$, then, by (17),

$$\left|R_n(x;f)\right| < \tau E_{n-1}(f) \log n \ (a \leqq x \leqq b).^{3}$$

We now take up the question S: *is LIP stable*? The answer is in the negative: *LIP is unstable, no matter how the points* $\{x_{i,n}\}$ *are chosen.* In fact, by virtue of (18), for at least one point in (a,b)

[3] Hereafter τ generally stands for a finite quantity, independent of n or x (different, of course, in different formulae).

$$F_n(\xi_n) = \sum_{i=1}^{n} \left| \phi_{i,n}(\xi_n) \right| > A \quad (n > N), \tag{20}$$

where A designates an arbitrarily large positive quantity. Consider now (7) with

$$\epsilon_{i,n} = \pm \epsilon, \ \epsilon_{i,n}\phi_{i,n}(\xi_n) > 0 \qquad (i = 1, 2, \cdots, n), \tag{21}$$

and we get, in view of (20, 21), the desired proof of instability:

$$\left| \Delta\Phi_n \right|_{x=\xi_n} = \epsilon \sum_{i=1}^{n} \left| \phi_{i,n}(\xi_n) \right| > \epsilon A (n > N).$$

Moreover, this *exaggeration of observational errors* (in the functional values $f(x_{i,n})$) is very grave. In fact, it cannot be made to disappear by taking a more regular $f(x)$ (for $\Delta\Phi_n$ is independent of $f(x)$), and it takes place not only at isolated points, but on intervals whose total length is *finite*. Fréchet, for example, has shown [4] (cf. also [1, 2, 3]), in case of equidistant $x_{i,n}$, that if one of the functional values $f(x_{i,n})$ is affected by a small error ϵ, the corresponding error $|\Delta\Phi_n|$ in $\Phi_n(x; f)$, for n sufficiently large, can be made arbitrarily large over a set of intervals, the total length of which becomes as close as we please to $\frac{1}{2}$ of the length of the interval (a,b) in question.

The fact that LIP is neither convergent[4] *nor stable calls for extreme caution in its use.*

5. *Stability and convergence of a polynomial interpolation formula.*— Assume that (1), where $\phi_{i,n}(x)$ are polynomials, possesses the property (12) and also the following:

$$\phi_{i,n}(x) \geqq 0 \text{ for } a \leqq x \leqq b (i = 1, 2, \cdots, n; n > N). \tag{22}$$

Then (9) is also satisfied. Hence,

Theorem I. Under the conditions (12, 22) *formula* (1) *is stable (for* $n > N$).

It is also easy to prove

Theorem II. Formula (1) *is convergent at a certain point* x *in* (a,b), *if it possesses the properties* (12, 22), *and, at the point* x, *the following*:

$$\lim_{n\to\infty} \sum_{|x-x_{i,n}|>\delta} \phi_{i,n}(x) = 0 \qquad (\delta > 0). \tag{23}$$

It converges uniformly in any interval $(c,d) \subset (a,b)$, *in which* (23) *holds uniformly.*

The second part of our statement results at once from the proof which we proceed to give for the first part.

[4] Even for the subclass of analytic functions [5].

Rewrite (13) as

$$R_n(x;f) = \sum_{|x-x_{i,n}|\leq\delta} \phi_{i,n}(x)[f(x) - f(x_{i,n})] + \sum_{|x-x_{i,n}|>\delta} id. \equiv \sum_1 + \sum_2.$$

By taking, first, $n>N$ sufficiently large, then $\delta>0$ sufficiently small, we have, denoting by $\epsilon=\epsilon(n, \delta)$ a suitable small positive quantity which $\to 0$ as $n\to\infty$, $\delta\to 0$, making use of (23) and the continuity of $f(x)$,

$$\left|\sum_{|x-x_{i,n}|>\delta} \phi_{i,n}(x)\right| \leqq \frac{\epsilon}{2M} (n > N;\ M = \max|f(x)| \text{ in } (a, b))$$

$$|f(x) - f(x_{i,n})| \leqq \frac{\epsilon}{2} \qquad (|x - x_{i,n}| \leqq \delta).$$

On the other hand, by (12, 22),

$$\left|\sum_{|x-x_{i,n}|\leq\delta} \phi_{i,n}(x)\right| \leqq \sum_{i=1}^{n} \phi_{i,n}(x) = 1.$$

Hence,

$$\begin{aligned} |R_n(x;f)| &\leqq |\textstyle\sum_1| + |\sum_2| \\ &\leqq \frac{\epsilon}{2} \sum_{|x-x_{i,n}|\leq\delta} \phi_{i,n}(x) + 2M \sum_{|x-x_{i,n}|>\delta} \phi_{i,n}(x) \\ &\leqq \frac{\epsilon}{2}\cdot 1 + 2M\cdot\frac{\epsilon}{2M} \doteq \epsilon.^{8} \end{aligned}$$

Corollary.—A polynomial interpolation formula possessing the properties (12, 22, 23) *for all* x *in* (a,b) *and all* n, *is both stable and convergent.*

6. *Fejér's interpolation formula* [6, 14, 15].—It is constructed as follows (we write, for brevity, x_i in place of $x_{i,n}$):

$$\Phi_n(x;f) = \sum_{i=1}^{n} \left[1 - (x - x_i)\frac{\phi''(x_i)}{\phi'(x_i)}\right]\cdot\left[\frac{\phi(x)}{(x - x_i)\phi'(x_i)}\right]^2 f(x_i)$$

$$\equiv \sum_{i=1}^{n} \phi_{i,n}(x) f(x_i) \left(\phi(x) = \prod_{i=1}^{n} (x - x_i)\right). \tag{24}$$

We see readily that

$$\Phi_n(x_i;f) = f(x_i),\ \Phi_n'(x_i, f) = 0\ (i = 1, 2, \cdots, n), \tag{25}$$

[8] The reader will notice that we have used here the continuity of $f(x)$ at the point x only. *Our results, properly modified, thus hold for bounded functions, continuous on a part of* (a,b) *only.*

so that *(12) is satisfied.* Taking $(a,b) \equiv (-1, 1)$, Fejér further chooses the x_i as zeros of Legendre polynomial

$$P_n(x) = \frac{1}{n!\,2^n} \frac{d^n}{dx^n}[(x^2 - 1)^n],$$

which are known to be distinct and to lie between -1 and 1. Making use of the differential equation

$$(26) \qquad (1 - x^2)P_n''(x) - 2xP_n'(x) + n(n + 1)P_n(x) = 0$$

and of some other properties of $P_n(x)$ (as, for example, $P_n(1) = 1$, $|P_n(x)| \leqq 1$ for $-1 \leqq x \leqq 1$, $|P_n(x)| < (\tau/\sqrt{n(1-x^2)})$ for $-1+\delta \leqq x \leqq 1-\delta$), Fejér shows that (22) is satisfied for all n, and (23) holds for any x inside $(-1, 1)$. Hence, the conclusion: *Fejér's interpolation formula* (24) *is stable and converges uniformly over any interval lying wholly inside* $(-1, 1)$.

7. *Other stable and convergent interpolation formulae.*—The above notion of stability can be somewhat modified by imposing on $\Phi_n(x; f)$ in (10) the condition of possessing the following property:

S'. $|f_1(x) - f_2(x)| \leqq \epsilon$ *for* $a_1 \leqq x \leqq b_1$ *implies* $|\Phi_n(x; f_1) - \Phi_n(x; f_2)| < t\epsilon$ *for* $n > N$, $a_1 \leqq x \leqq b_1$, $f_{1,2}(x)$ *being any two members of the class* $[f]$, t *being independent on* n, x, ϵ, $(a_1, b_1) \subset (a,b)$.

The writer [12] has constructed polynomial interpolation formulae of Fejér's type (24), using for $\phi(x)$ the so-called *classical orthogonal polynomials* (of which Legendre polynomial is a particular case) of

$$\text{Jacobi:} \quad C(x - a)^{1-\alpha}(b - x)^{1-\beta} \frac{d^n}{dx^n}[(x - a)^{n+\alpha-1}(b - x)^{n+\beta-1}]$$

$$(\alpha, \beta > 0;\ (a, b) \text{ finite}),$$

$$(27) \quad \text{Laguerre:} \ Cx^{1-\alpha}e^x \frac{d^n}{dx^n}[e^{-x}x^{n+\alpha-1}] \qquad (\alpha > 0;\ (a, b) \equiv (0, \infty)),$$

$$\text{Hermite:} \ Ce^{x^2} \frac{d^n(e^{-x^2})}{dx^n} \qquad ((a, b) \equiv (-\infty, \infty))$$

$$(C = \text{const.}).$$

Making use of the differential equation of the type (26)

$$A(x)y'' + B(x)y' + C_n y = 0$$

$$(A,B - \text{polynomials}, C_n = \text{const.}),$$

satisfied by the polynomials (27), of their asymptotic expressions for $n \to \infty$ and of some results in the theory of mechanical quadratures, it

was possible to show that (24), *constructed with the aid of the polynomials* (27), *possesses stability and convergence in properly specified intervals, although* (22) *is not always satisfied.*[6]

8. *Jackson's trigonometric interpolation formula.*—The above results hold, *mutatis mutandis,* for trigonometric interpolation as well. Without entering into details, we give here an interesting formula due to D. Jackson [7], in which $f(x)$ is assumed to have the period 2π:

$$(28)\qquad \Phi_n(x;f) = \frac{1}{n^2}\sum_{i=1}^{n}\left[\frac{\sin(n/2)(x-x_i)}{\sin\frac{1}{2}(x-x_i)}\right]^2 f(x_i) \qquad \left(x_i = \frac{2i\pi}{n}\right),$$

where each term is so defined as to be continuous whenever its denominator vanishes. One verifies readily that (28) is a trigonometric sum of order $n-1$, and that

$$(29)\qquad \Phi_n(x_i;f) = f(x_i)(i=1, 2, \cdots, n);\ \frac{1}{n^2}\sum_{i=1}^{n}\left[\frac{\sin(n/2)(x-x_i)}{\sin\frac{1}{2}(x-x_i)}\right]^2 \equiv 1.$$

Thus, (28) is an interpolation formula for which (12) and obviously (22) are satisfied. That (23) also holds, is seen from

$$\frac{1}{n^2}\sum_{|x-x_i|>\delta}\left[\frac{\sin(n/2)(x-x_i)}{\sin\frac{1}{2}(x-x_i)}\right]^2 \leqq \frac{1}{\sin^2(\delta/2)}\cdot\frac{1}{n^2}\sum_{i=1}^{n}1$$

$$= \frac{1}{n\sin^2(\delta/2)} < \epsilon \qquad (n > N).$$

Hence, the conclusion: *Jackson's trigonometric interpolation formula* (28) *is stable and convergent.*

Remark. Convergence and stability are a good compensation for the too high degree, order of $\Phi_n(x;f)$ in the formulae (24, 28) respectively of Fejér and Jackson, as compared to the number of points x_i where $\Phi_n(x;f)$ coincides in value with $f(x)$.

9. *Bernstein's limiting interpolation formula.*—We close our discussion with the following formula due to S. Bernstein [9]:

$$(30)\qquad \Phi_n(x;f) = \sum_{i=0}^{n}\binom{n}{i}x^i(1-x)^{n-i}f\left(\frac{i}{n}\right) \equiv \sum_{i=0}^{n}\phi_{i,n}(x)f\left(\frac{i}{n}\right)$$

$$(a, b) \equiv (0, 1), \quad x_{i,n} = \frac{i}{n}, \quad \phi_{i,n}(x) = \binom{n}{i}x^i(1-x)^{n-i}.$$

This is not, strictly speaking, an interpolation formula, for (3) is not

[6] When using in (24) the polynomials of Laguerre or Hermite, the function to be interpolated is assumed to satisfy certain conditions at infinity.

satisfied. It may be called a "limiting interpolation formula," for if we write $G_s(x)$ as

$$G_s(x) = \sum_{i=0}^{n} \gamma_i x^i (1-x)^{n-i},$$

then,

$$\lim \frac{\gamma_i}{\binom{n}{i}} = \lim G_s\left(\frac{i}{n}\right) (n \to \infty).$$

Formula (30) *is both stable and convergent in* (0, 1). Stability is insured by the fact that (12, 22) are satisfied.[7] Convergence was established by Bernstein (using Weierstrass' Approximation theorem, or resorting to (23)). Furthermore, it was proved recently [10, 11] that, if $f(x)$ is continuously differentiable in (0, 1) up to a certain order s inclusive, then, uniformly in (0, 1),

$$f^{(k)}(x) = \lim_{n \to \infty} \Phi_n^{(k)}(x; f) \qquad (k = 1, 2, \cdots, s).$$

The above properties of Bernstein's formula (30) and its simple structure make it a very useful tool in applications.

The University of Pennsylvania

BIBLIOGRAPHY

1. Hahn, Über das Problem der Interpolation, *Math. Zeitschrift*, I (1918), 115–143.

2. Borel, *Leçons sur les fonctions d'une variable réelle* (Paris, Gauthier-Villars, 1904), Ch. IV.

3. Ch.-J. de la Vallée Poussin, Sur la convergence des formules d'interpolation entre ordonnées équidistantes, *Bull. de l'Acad. r. de Belgique, Cl. des Sciences* (1908), 319–403.

4. Fréchet, Sur un défaut de la méthode d'interpolation par les polynomes de Lagrange, *Nouv. Annales de Math.* (4), XX (1920), 1–9.

5. Runge, Über empirische Funktionen und die Interpolation zwischen equidistanten Ordinaten, *Zeitschrift fur Math. und Phys.*, XLVI (1902).

6. Fejér, Über Interpolation, *Göttinger Nachrichten* (1916), 66–91.

7. D. Jackson, "A Formula of Trigonometric Interpolation," *Rendic. Circ. Mat. di Palermo*, XXXVI (1913), 371–375.

8. S. Bernstein, Quelques remarques sur l' interpolation, *Commun. de la Soc. Math. de Kharkoff*, XIV (1914), 1–13.

9. S. Bernstein, *On the Best Approximation of Continuous Functions by Means of Polynomials of Given Degrees* (Kharkoff, 1912, in Russian), 120–125.

10. S. Wigert, Sur l'approximation par polynomes des fonctions continues, *Ark. Mat. Astron. Fys.*, 22B, Nr. 9 (1932), 1–4.

[7] $\sum_{i=0}^{n} \binom{i}{n} x^i (1-x)^{n-i} \equiv [x+(1-x)]^n \equiv 1.$

11. I. Khlodowsky, "On Certain Properties of the Polynomials of S. Bernstein" (read before the Mathematical Congress in USSR, 1930), Congress Bull., No. 1, p. 49 (in Russian).

12. J. Shohat, On Interpolation, to appear in the *Annals of Mathematics*.

13. Faber, Über die Interpolationische Darstellung stetiger Funktionen, *Jahresber. der Deutch. Math. Ver.*, XXIII (1914), 192–210.

14. L. Fejér, Über Weierstrassche Approximation, besonders durch Hermitesche Interpolation, *Math. Annalen*, CII (1930), 707–725.

15. L. Fejér, Lagrangesche Interpolation und die zugehörigen konjugierten Punkte, *Math. Annalen*, CVI (1932), 1–55.

DISTRIBUTIONS STATISTIQUES RATTACHÉES A LA LOI DE GAUSS ET LA RÉPARTITION DES REVENUS

PAR G. DARMOIS

Communication présentée à la réunion de la Société d'Économétrie, Lausanne, septembre, 1931

LE BUT de cette communication est de signaler un travail étendu de R. Gibrat sur "les inégalités économiques"[1] où se trouvent représentées de façon qui paraît nouvelle, des répartitions de revenus, de fortunes, d'entreprises d'après le nombre des ouvriers, de villes par le nombre des habitants, etc. . . .

Nature des distributions statistiques utilisées.—L'importance des distributions de Laplace-Gauss a conduit depuis longtemps à considérer les distributions qu'on en déduisait par un changement quelconque de la variable indépendante (method of translation of Edgeworth). Kapteyn[2] et Wicksell[3] les obtiennent par la théorie des probabilités. R. Gibrat utilise tout particulièrement le cas où c'est la variable:

$$y = a \log (x - x_0) + b,$$

qui se trouve distribuée suivant une loi de Gauss; disons pour abréger que ce sera une loi A. La détermination des constantes dans un cas concret ne présente aucune difficulté, on est ramené à examiner si des points se placent convenablement sur une droite, et à déterminer ensuite la droite qui s'ajuste le mieux. La qualité de la représentation obtenue peut alors s'étudier par les méthodes inaugurées par K. Pearson, en tenant compte du nombre des paramètres employés pour l'ajustement.

La distribution des revenus.—On sait que la représentation se fait généralement par les formules de Pareto, le nombre N des revenus supérieurs à x ayant, soit la forme simple $Ax^{-\alpha}$, ou les formes plus compliquées,

$$Ax^{-\alpha}e^{-\beta x}, \qquad A(x+a)^{-\alpha}e^{-\beta x}.$$

Ces formules sont empiriques. (On peut signaler toutefois un travail de F. P. Cantelli[4] où la loi de Pareto est déduite, à partir de certaines

[1] Librairie du Recueil Sirey, 1931.

[2] "Skew frequency curves in biology and statistics," (*Public. du Labor. de Gröningen.* 1903).

[3] "On the genetic theory of frequency," *Arkiv. für mat. astro. fys.*, Bd. 12. No. 20.

[4] "Sulla legge di distribuzione dei redditi," *Giornale degli Economisti e Rivista di Statistica*, novembre, 1929.

hypothèses, comme la loi de répartition ayant la probabilité la plus grande.)

La loi de la forme la plus simple est généralement suffisante. La forme la plus compliquée n'a été jugée nécessaire, par Pareto, que pour la représentation des revenus du grand Duché d'Oldenbourg en 1890. R. Gibrat a cherché à représenter des distributions de revenus par la loi A. Les résultats sont très satisfaisants. Parmi les nombreux exemples qu'on trouve dans l'ouvrage cité, le plus frappant peut-être est celui des revenus d'Oldenbourg; la formule de Pareto à quatre paramètres fournissant 8 per cent d'erreur moyenne, tandis que la formule du type *A* ne donne que 2 per cent (page 124). On peut donc considérer, du simple point de vue de l'ajustement, ces distributions comme très utiles.

Représentation d'autres données statistiques.—Les formules du type A peuvent également s'appliquer de façon satisfaisante aux fortunes (pp. 139 et suiv.), aux salaires (p. 179), aux villes suivant la population (p. 251), etc.

Indice d'inégalité.—De nombreux indices ont été proposés pour caractériser numériquement l'inégalité des revenus. Il faut ajouter aux travaux cités par Gibrat l'important mémoire de C. von Bortkievicz[5] qui met au point cette question. Il est naturel de penser que, dans les cas où les lois de type *A* s'appliquent, l'écart type de la loi de Gauss doit être un élément fondamental. On constate en effet que les différentes définitions proposées conduisent, soit rigoureusement, soit d'une manière approchée, à utiliser de simples fonctions de cet écart type. On obtient aussi un indice d'inégalité fourni directement par l'ajustement d'une droite aux points déduits des observations.

L'effet proportionnel.—L'utilité des lois du type *A* pour la représentation analytique de données statistiques est une chose, l'interprétation éventuelle de ces lois en est une autre. On sait, par exemple, que la répartition de certaines grandeurs appartenant à la physique, à la biométrique, ou à la psychologie expérimentale, se conforme assez bien au type de Laplace-Gauss. D'autre part, des types voisins de celui-là peuvent être obtenus par la sommation d'un grand nombre de variables aléatoires, sous des conditions extrêmement générales. La réussite d'une loi de ce type dans la représentation des observations conduit à examiner ce qu'il peut y avoir de concret dans cette sommation d'effets élémentaires.

Les cas de mesures précises effectuées sur une grandeur physique appartiennent visiblement à ce type. La grandeur fluctue autour d'une

[5] "Die disparitätsmasse der Einkommen statistik." (La Haye, 1930.) *XIX^e Session de l'Institut International de Statistique.*

valeur moyenne, sous l'influence de causes aléatoires nombreuses, et réalise concrètement le schema précédent.

La distribution des tailles d'un groupe d'individus peut être rattachée d'une manière assez concrète à cet ordre d'idées.

Les lois du type A se rattachent également à une sommation d'effets-aléatoires mais en admettant que l'effet sur la grandeur mesurée soit proportionnel à la cause aléatoire, le coefficient étant lui-même proportionnel à la grandeur mesurée à partir d'une valeur x_0. Dans ces conditions, c'est le logarithme de $x-x_0$ qui résulte d'une sommation de variables aléatoires et suit en conséquence une loi voisine de celle de Gauss.

Ce fait que la grandeur réagit à une cause, par une variation proportionnelle à $x-x_0$, est ce que Gibrat appelle l'effet proportionnel.

L'effet proportionnel conduit donc aux lois du type A. La réciproque ne s'impose nullement, bien entendu, et s'il est raisonnable d'y penser, elle ne peut être établie que par une étude directe. Ce n'est, dans les cas favorables, que l'ébauche d'une théorie statistique.

Si, par exemple, l'idée de l'effet proportionnel paraît naturelle pour les grandes villes, ce ne peut être qu'une approximation pour les revenus.[6]

Université de Nancy

[6] Des résultats analytiques semblables ont été obtenus par une voie toute différente dans les travaux de J. Van der Wijk—"Revenu psychique et fortune psychique," *Revue hollandaise—Overdruk Economist*—1931.

INTERRELATIONS DES PRIX ET COURBES STATISTIQUES DE DEMANDE ET D'OFFRE

PAR MARCO FANNO

Communication présentée à la réunion de la Société d'Économétrie, Lausanne, septembre 1931

1°. LE BUT de cette communication est d'examiner les problèmes que l'on rencontre dans la construction des courbes statistiques de demande et d'offre, à cause des rapports d'interdépendance existant entre les différents prix. En théorie l'on peut admettre que tous les prix sont interdépendants. En pratique toutefois l'on peut considérer interdépendants (en telle mesure au moins qu'il vaille la peine d'en tenir compte) seulement les prix des biens qui se trouvent entre eux en relations économiques directes. D'après la nature de ces relations les différents biens peuvent se diviser en groupes différents, dont les plus importants sont les cinq suivants: (1) groupe de biens à production associée; (2) groupe de biens de remplacement; (3) groupe de biens complémentaires; (4) groupe de biens concurrents du côté de l'offre; (5) groupe de biens liés entre eux par des rapports d'instrumentalité. Nous allons nous occuper des deux premiers groupes seulement. Mais les conclusions auxquelles nous arriverons sont suffisantes à nous montrer la complexité de ces relations et la nécessité d'en tenir compte, si possible, dans la construction des courbes de demande et d'offre.

2°. En ce qui concerne le premier groupe il faut d'abord distinguer les biens à production associée qui sont produits à rapport fixe, de ceux qui sont produits à rapport variable. Dans le premier cas l'équilibre du marché de ces biens et les changements de cet équilibre dépendent des conditions techniques qui déterminent le rapport des quantités des deux biens. Dans le deuxième cas ils dépendent des conditions qui déterminent le rapport des quantités le plus avantageux pour les producteurs.[1] En régime de libre concurrence parfaite, l'équilibre se réalise toujours en relation au principe du coût de production, mais avec cette différence, en comparaison avec les cas plus simples considérés par les économistes, que l'égalité doit se réaliser non pas entre le prix et le coût marginal de chaque marchandise, mais entre le coût marginal global et la somme des prix des fractions d'unité des biens associés que l'on peut obtenir de chaque unité de production

[1] Edgeworth, *The Economic Journal*, juin 1915, pp. 197 à 200; Edgeworth, *Papers relating to Political Economy* (London, 1925), II, 486 ss.; Fanno, "Contributo alla teoria dell'offerta a costi congiunti," *Giornale degli Economisti*, octobre 1914.

globale. En partant d'une position d'équilibre qu'on peut considérer comme déjà réalisée, et en supposant qu'il arrive un changement de la demande (dans le sens d'un *shifting* de la courbe de la demande),[2] et par conséquent du prix d'un des produits associés, le prix de l'autre ou des autres biens du même groupe changera en direction opposée de celle du premier. Cela arrive parce que si le prix d'un de ces biens hausse, la production globale, et, par conséquent, la production de tous les biens du groupe, augmente aussi. Il dérive de tout cela une conséquence très importante à propos de la construction des courbes de l'offre de ces biens. Considérons, par exemple, le cas de deux biens associés: les marchandises A et B. En supposant maintenant que la courbe de la demande de A change lentement de position dans une direction donnée, son prix changeant aussi graduellement, il se produira alors un changement graduel: (1) dans la production de A; (2) dans la production de B; (3) dans le prix de B; et avec les données de la production et des prix des deux marchandises que nous aurons à notre disposition, nous pourrons construire les courbes respectives de l'offre. Mais, tandis que la courbe de l'offre de A aura un coefficient d'élasticité positif, la courbe de B aura un coefficient négatif. La forme particulière indiquée de chacune des deux courbes dépend naturellement des conditions que nous avons supposées existantes. Si ces conditions changent et l'on suppose que le prix de B varie, il en résultera alors une courbe d'offre positive pour B et négative pour A. On peut donc conclure que la courbe de l'offre d'un bien, dont la production est associée à celle d'un ou d'autres biens, peut être tantôt positive, tantôt négative, selon que l'initiative du mouvement procède du prix de ce bien ou de celui des autres. Cependant dans la vie réelle il n'arrive presque jamais que l'initiative de la perturbation ait son origine exclusivement dans le prix d'un seul bien. Il arrive presque toujours que les prix des biens associés changent tous en même temps, en proportions différentes, et tantôt dans une direction, tantôt dans une autre. Les changements de la production de chacun de ces biens ne se produisent donc jamais selon les solutions extrêmes indiquées, mais ils se produisent comme résultante de l'action combinée des variations simultanées spontanées des différents prix; et leurs courbes d'offre peuvent conséquemment avoir les formes les plus diverses. La conclusion que l'on en tire est la suivante: que si l'on veut déterminer la nature des relations entre les prix et les quantités produites des biens

[2] A propos de la représentation graphique et de la signification économique du changement de position des courbes de la demande voir: Cunningham, *A Geometric Political Economy* (Oxford, 1904), p. 78; Marshall, *Principles of Economics* (5e éd., pp. 97–98, note 2); Fisher, *Elementary Principles of Economics* (New York, 1918), pp. 268 ss.

de ce groupe, il ne suffit pas d'étudier chaque bien séparément, mais il faut étudier tous les biens du même groupe à la fois, en tenant compte des variations simultanées de leurs prix.

3°. Les corrélations entre les prix des biens de remplacement dérivent des liens qui existent entre leurs demandes. Les prix des biens de ce groupe changent dans la même direction[3]; mais la mesure de leurs changements respectifs varie selon qu'il s'agit de biens qui se remplacent les uns les autres à rapport marginal fixe ou à rapport marginal variable. Les premiers sont des biens de remplacement parfaits, et leurs lignes d'indifférence des goûts (dans le sens de Edgeworth et de Pareto) sont des droites; les seconds sont des biens de remplacement imparfaits, dont les lignes d'indifférence sont des courbes concaves. En partant toujours d'une position d'équilibre, que nous supposons déjà réalisée, et en imaginant que la courbe de la demande d'un de ces biens change de position et que son prix hausse, nous pouvons énoncer le principe suivant: lorsqu'il s'agit de biens à rapport de remplacement fixe à une augmentation donnée du prix d'un d'eux s'accompagne une augmentation proportionnelle des prix des autres biens; dans le cas de biens à rapport de remplacement variable la hausse du prix ou des prix des autres biens n'est pas proportionnelle à la hausse du premier. Voyons-en les conséquences.

Si le prix d'un bien hausse, les prix des autres biens haussent aussi, parce qu'une partie de la demande du premier se transfère aux seconds. Dans l'hypothèse que les biens de remplacement soient les deux marchandises A et B, nous pouvons imaginer, comme dans le cas précédent, que la courbe de la demande de A change graduellement de position et que son prix hausse graduellement aussi. Il en suivra: (1) une diminution de la demande de A; (2) une augmentation de la demande de B; (3) une hausse du prix de B. Et au moyen des données des prix et des quantités demandées des deux biens nous pourrons construire leurs courbes de demande. Mais tandis que la courbe de la demande de A sera une courbe négative (d'après la terminologie de Marshall) celle de B sera une courbe positive (cela naturellement dans l'hypothèse que le prix de A a changé le premier). Si l'on suppose maintenant que le prix de B change le premier, la forme des deux courbes sera invertie. L'on en déduit que la forme des courbes de la demande des biens de remplacement n'est pas toujours la même, mais elle peut être tantôt positive, tantôt négative, selon que, en hypothèse, l'on imagine que la perturbation procède exclusivement du prix de l'un des biens ou de celui de l'autre. Il est évident, d'ailleurs, que cela

[3] Fanno, "Contributo alla teoria economica dei beni succedanei," *Annali di Economia*, 1926; et "Die Elastizität der Nachfrage nach Ersatzgütern," *Zeitschrift für Nationaloekonomie*, mai 1929.

n'arrive jamais dans la vie réelle, tous les prix étant destinés à changer continuellement de leur propre initiative dans une direction ou dans une autre. Il en résulte que les relations entre la demande et les prix des biens de remplacement ne correspondent presque jamais aux deux solutions extrêmes indiquées, mais sont généralement la résultante des changements autonomes simultanés de leurs prix. L'on peut conclure donc, même pour ce groupe de biens, que si l'on veut étudier à fond lesdites relations, il ne suffit pas de les étudier pour chaque bien séparément, mais il est nécéssaire de prendre en considération les variations simultanées des prix de tous les biens du groupe. On pourrait faire des considérations analogues à propos des trois autres groupes de biens. Mais ce que nous venons d'exposer à propos des deux premiers est suffisant à notre but.

4°. Les conclusions auxquelles nous sommes arrivés se rattachent directement au problème des courbes statistiques de demande et d'offre, problème qui est maintenant à l'ordre du jour, mais dont, il ne faut pas l'oublier, s'est occupé dès 1907 le professeur Benini, le maître des statisticiens italiens.[4]

On a objecté que ces courbes ne sont pas des véritables courbes de demande et d'offre telles qu'elles sont étudiées par l'économie pure, parce que, tandis que celles-ci se rapportent aux conditions d'un moment donné, celles-là se rapportent aux variations moyennes de la demande et de l'offre le long d'une période de temps donnée (Amoroso, Moretti);[5] qu'elles sont donc simplement des courbes historiques, représentant les sentiers parcourus, dans une période de temps donnée, par les points successifs d'équilibre d'un marché (Amoroso, Waterman-Gilboy),[6] et que comme telles chacune d'elles peut être considérée en même temps une courbe de demande et une courbe d'offre.[7] Ces objections sont certainement très graves, mais elles se rapportent à ces courbes seulement si on les considère comme un moyen de vérification des lois de l'économie pure; mais elles perdent toute leur valeur, si l'on considère ces courbes ce qu'elles sont simplement, à savoir, "la représentation graphique des relations entre les prix et les quantités de-

[4] Benini, "Sull'uso delle formule empiriche nell'Economia applicata," *Giornale degli Economisti*, 1907; et "Una possibile creazione del metodo statistico: L'Economia Politica induttiva," *Giornale degli Economisti*, 1908. Voir aussi Gini, "Prezzi e Consumi," *Giornale degli Economisti*, 1910.

[5] Amoroso, "Le equazioni differenziali della dinamica economica," *Giornale degli Economisti*, février 1920, p. 71; Moretti, "Sopra alcuni problemi di dinamica economica," *Giornale degli Economisti*, juillet 1929, p. 474.

[6] Amoroso, *loc. cit.* p. 73; Waterman-Gilboy, "Demand Curves in Theory and in Practice," *The Quarterly Journal of Economics*, août 1930, p. 610.

[7] Working, "What do Statistical 'Demand Curves' Show?" *The Quarterly Journal of Economics*, février 1927, p. 222.

mandées et offertes des différents biens." Voyons donc quels sont les problèmes principaux que l'on rencontre à l'occasion de leur construction.

Puisque l'on ne peut pas disposer de données relatives au même moment, l'on doit construire ces courbes au moyen de données relatives à des moments successifs, données qui sont affectées par les perturbations des facteurs qui agissent dans le temps, comme les variations de la population, des consommations, du revenu national, du pouvoir d'achât de la monnaie, des goûts des consommateurs, etc. Le statisticien qui s'applique à ce genre de travail doit donc éliminer d'abord les perturbations de ces différents facteurs,[8] et pour y parvenir il n'a qu'à appliquer l'un ou l'autre des différents procédés que la science moderne a mis à sa disposition.[9]

Mais, ces premières difficultés surmontées, d'autres se présentent par suite des interrélations entre les différents prix. Nous avons vu qu'à cause des relations d'interdépendance existant entre les biens à production associée et les biens de remplacement (et les mêmes considérations pourraient se répéter pour les autres groupes de biens) la demande et l'offre de chaque bien ne se comportent pas toujours de la même manière en relation aux variations du prix, mais elles se comportent de manière différente selon que les prix des biens du même groupe changent, ou ne changent pas, en même temps de leur propre initiative, selon qu'ils changent plus ou moins sensiblement. Si après avoir éliminé l'influence des facteurs perturbateurs indiqués l'on se bornait donc à tracer les courbes de demande et d'offre d'un bien en ne tenant compte que des variations de son prix, l'on parviendrait sans doute à déterminer les variations que la demande et l'offre de ce bien ont subi simultanément aux variations données de son prix, mais l'on ne parviendrait pas à déterminer quelle est l'influence réelle exercée sur ces variations par les variations du prix du bien dont il est question. Pour

[8] Marshall, *Principles of Economics* (V° Edit., London, 1907), p. 109; Pigou, *The Economics of Welfare* (London, 1924), p. 736.

[9] Moore, "A Theory of Economic Oscillations," *Quarterly Journal of Economics*, novembre 1926; Moore, *Synthetic Economics* (New York, 1929) p. 42; Schultz, "The Statistical Law of Demand," *The Journal of Political Economy*, octobre et décembre 1925; Schultz, *The Meaning of Statistical Demand Curves* (Chicago, 1930); Leontief, "Ein Versuch zur Statistischen Analyse von Angebot und Nachfrage," *Weltwirtschaftliches Archiv*, juillet 1929; Pigou, "The Statistical Derivation of Demand Curves," *The Economic Journal*, septembre 1930; Brescian-Turroni, *Relations entre la récolte et le prix du coton Egyptien* (Le Caire, 1930), p. 655, et "Über die Elastizität des Verbrauchs ägyptischer Baumwolle," *Weltwirtschaftliches Archiv*, janvier 1931, p. 69; Staehle, *Die Analyse von Nachfragekurven in ihrer Bedeutung für die Konjunkturforschung* (Bonn, 1929); Waterman-Gilboy, "The Leontief and Schultz Methods of Deriving Demand Curves," *The Quarterly Journal of Economics*, février 1931.

y parvenir il est nécessaire d'éliminer des données disponibles l'influence perturbatrice des autres prix.

En suivant le procédé indiqué pour d'autres buts par Marshall,[10] quelques auteurs conseillent de réunir ensemble les biens appartenant à chaque groupe et de les considérer comme une seule marchandise.[11] C'est sans doute un procédé simplificateur; mais (en faisant même abstraction de ce qu'il ne peut pas être appliqué à tous les groupes de biens) il évite le problème sans le résoudre. Au moyen de ce procédé l'on parvient à construire des courbes de demande et d'offre de groupes de biens, au lieu de courbes de biens particuliers; et, à cause de cela, l'on ne parvient pas à déterminer, par exemple, pour le groupe des biens de remplacement (ce qui est réellement important et intéressant) quelle est la réaction des consommateurs à la hausse du prix d'un de ces biens, et dans quelle mesure le public, en conséquence d'un changement de leurs prix, remplace la consommation des biens de première qualité par celle des biens de qualité inférieure ou *vice versa*.

Pour résoudre le problème il faut donc l'attaquer en plein; il se divise alors en deux autres problèmes: (1) le problème relatif au choix des procédés les plus convenables pour l'élaboration de chaque groupe de biens; (2) le problème relatif à la détermination du nombre des groupes de biens auxquels ces procédés doivent être appliqués dans les différents cas.

5°. En ce qui concerne le premier problème nous nous bornerons à l'étudier en relation seulement avec les deux groupes de biens dont nous nous sommes occupés.

A propos des biens de remplacement nous avons vu que leurs prix changent dans la même direction, et que, si l'un d'eux seul change de sa propre initiative, l'autre, ou les autres, le suivent passivement, et changent dans la mesure correspondante à leur rapport de corrélation. Il en suit que les variations de la demande du deuxième bien (que j'indiquerai comme auparavant par B), quoiqu'elles puissent apparaître déterminées par les variations de son prix, sont en réalité déterminées par les variations du prix de l'autre bien que j'indiquerai par la lettre A. Dans la vie réelle, toutefois, le prix de B a aussi des mouvements autonomes, qui déterminent des changements de la demande et des mouvements dérivés du prix de A. L'on en déduit: (1) que les changements auxquels le prix de B est assujetti sont en partie

[10] Marshall, *Principles ec.* p. 391 et Mathematical Appendix xx p. 854.

[11] Ce procédé fut appliqué dès 1918 par le prof. Vinci. Voir Vinci, "L'elasticità dei consumi," *L'Economista* 1918, réimprimé récemment dans la *Rivista Italiana di Statistica*, janvier-mars 1931, pp. 52–53 et 66. Il a été proposé aussi par Pigou, *The Economics of Welfare*, p. 739. Voir aussi Marschak, *Elastizität der Nachfrage* (Tübingen, 1931), p. 62.

des changements *dérivés* des variations du prix de A, et en partie des changements *autonomes* du prix même; (2) que les variations simultanées des quantités demandées de B sont en partie dépendantes *des changements directs du prix de A et dérivés du prix de B, et en partie des variations directes du prix de B et dérivées du prix de A*. A propos des biens de remplacement le premier problème est donc de trouver le moyen: (1) d'éliminer des variations du prix de B celles dûes aux variations autonomes du prix de A, de manière à isoler les variations autonomes du prix de B; (2) d'éliminer des variations de la demande de B celles dûes aux changements autonomes du prix de A, de manière à isoler celles dûes aux variations autonomes du prix de B. Un problème analogue se présenterait si nous voulions construire la courbe de la demande de A. L'élimination des interférences dont nous avons parlé a toutefois une importance différente selon l'importance relative des biens considérés. Si, par exemple, le bien A est très important, c'est à dire un bien dont la production et consommation sont remarquables, tandis que le bien B est peu important, il est évident que le prix de B ne peut exercer qu'une influence insensible sur le prix de A. Dans ce cas il serait indifférent, en ayant à construire la courbe de la demande de A, d'éliminer ou de ne pas éliminer l'influence des mouvements autonomes du prix de B. Mais si l'on doit au contraire construire la courbe de la demande de B, on ne peut pas s'abstenir d'éliminer l'influence des mouvements autonomes du prix de A. En général on peut en conclure que l'élimination de l'influence réciproque des mouvements autonomes des prix est nécéssaire pour les deux biens lorsqu'ils ont à peu près la même importance et sont des biens de remplacement parfaits.

En ce qui concerne les biens à production associée nous avons vu que les changements du prix de B dépendent en partie de ses variations spontanées et en partie des variations de la production de B, dépendentes des variations de la production de A, qui sont déterminées à leur tour par les variations du prix de A. A propos de ce groupe de biens le premier problème est de trouver le moyen: (1) d'éliminer des variations du prix de B celles qui sont dûes aux variations de la production de B, déterminées des variations *autonomes* du prix de A, de manière à isoler les variations *autonomes* du prix de B; (2) d'éliminer des variations de la production de B celles qui dérivent des variations autonomes du prix de A, de manière à isoler les variations de la production de B, dûes exclusivement aux variations autonomes de son prix. Même pour ce groupe de biens l'élimination de ces interférences a une importance différente selon l'importance relative des deux biens. Si le bien B est peu important, parce qu' en produisant A l'on en produit une petite quantité et son prix est bas, une variation du prix de

B ne peut déterminer qu'une petite variation de la production globale de A et de B, et du prix de A. Si l'on doit construire la courbe de l'offre de A l'on peut donc s'abstenir d'éliminer l'influence des mouvements autonomes du prix de B. Mais si l'on doit construire la courbe de l'offre de B l'on ne peut pas faire de même pour le prix de A. Même pour ce groupe-ci l'on peut donc conclure que l'élimination de l'influence réciproque des mouvements autonomes des prix est indispensable pour *les deux biens* lorsque ils ont à peu près la même importance. Ce que nous avons exposé à propos de ces deux groupes peut se répéter *mutatis mutandis* à propos des trois autres groupes.

Mais de quelle manière peut-on éliminer, si nécessaire, entre chaque groupe, l'influence des interférences réciproques des prix? Les statisticiens conseillent *la méthode des corrélations multiples*,[12] ou d'autres méthodes analogues.[13] Dans les cas dans lesquels ces méthodes peuvent s'appliquer, le premier des deux problèmes énoncés peut donc se résoudre.

6°. Nous pouvons alors aborder le second. Considéré du point de vue d'un cas particulier celui-ci peut être énoncé de la manière suivante. Si l'on considère un bien quelconque qui appartienne, par exemple, au groupe des biens à production associée, est-il suffisant, pour construire la courbe de son offre, d'éliminer *seulement* l'influence des prix des biens de ce même groupe? Nous pouvons répondre tout court que, pour le cas considéré, c' est tout à fait insuffisant. Mais la même réponse vaut aussi pour tout autre cas. Et cela pour la raison suivante.

Les cinq groupes dans lesquels nous avons divisé les biens ne sont pas, comme nous les avons considérés jusqu'à présent, des groupes séparés, mais ils sont en effet des groupes unis les uns les autres et par conséquent interdépendants entre eux. Un bien n'appartient pas exclusivement à un seul groupe, mais il appartient en même temps à tous les cinq à la fois. Le blé est un bien de remplacement relativement au seigle, au maïs, à l'orge; mais il est en même temps un bien à production associée relativement à la paille, un bien complémentaire relativement à tous les autres biens nécessaires à la production de la farine, un bien rival du côté de l'offre relativement à tous les autres produits agricoles que l'on peut obtenir de la même terre, etc. Un changement du prix

[12] Yule, *An Introduction to the Theory of Statistics* (London, 1929); Moore, *Economic Cycles* (New York, 1914), pp. 81–82; Moore, *Forecasting the Yield and Price of Cotton* (New York, 1917). Le professeur Bresciani-Turroni a employé la méthode des corrélations multiples pour éliminer l'influence du prix du coton américain sur la valeur totale et sur le poids de la récolte du coton égyptien et déterminer le coefficient net de corrélation entre l'une et l'autre (voir *Relations entre la récolte ecc.* pages 683–4).

[13] Bresciani-Turroni, *L'uso dei metodi di correlazione nell'economia induttiva* (Palermo, 1912), p. 44.

du blé ne modifie donc pas seulement les prix des biens d'un seul groupe, mais il modifie en même temps les prix des biens de tous les cinq groupes, qui sont en relation directe avec le premier. Chaque bien est le centre d'une espèce de constellation qui forme un système solidaire de prix. Les changements de la demande ou de la production qu'un bien quelconque subit sur un marché dans une période donnée, ne dépendent donc pas seulement des variations de son prix et, par exemple, des prix des biens de remplacement direct; mais ils dépendent en même temps des variations de son prix et des variations des prix des biens de remplacement, des biens complémentaires, des biens associés, des biens rivaux du côté de l'offre, etc. C'est ainsi que, même en faisant abstraction du problème de l'interdépendance générale de tous les prix, et en se bornant à considérer pour chaque bien la seule influence des prix de ceux qui se trouvent en relations économiques directes avec lui, l'on doit reconnaître que la demande et l'offre de chaque bien sont influencées sensiblement, en même temps, par les prix d'un grand nombre de biens; de telle manière qu'ils sont en effet la résultante de l'action combinée des relations d'interdépendance entre tous les cinq groupes de biens réunis. Il est évident alors que, pour arriver à tracer d'une manière satisfaisante les courbes statistiques de la demande et de l'offre d'un bien quelconque, il n'est pas suffisant d'éliminer l'action perturbatrice des prix des biens d'un seul groupe, mais qu'il faut éliminer celle de tous les biens des différents groupes qui se trouvent en relation économique directe avec le premier et qui exercent une influence considérable sur son prix.

7°. Après tout ce que nous venons d'exposer dans les paragraphes ci-dessus nous sommes à même maintenant de percevoir d'un seul coup d'oeil tout le travail qu'exige la construction des courbes de demande et d'offre, surtout à cause de l'interdépendance entre les différents prix, et d'évaluer l'importance et les difficultés de ce travail. Après avoir éliminé la *tendance générale* des prix et des quantités, le statisticien qui, pour parvenir à des résultats rigoureusement exacts, trouve nécessaire de tenir compte de l'interdépendance entre les prix, doit avant tout déterminer la position économique du bien considéré, dans le but d'établir quels sont ses biens de remplacement, quels sont ses biens complémentaires, etc. Il devra en suite évaluer l'influence probable que les biens de chaque groupe exercent sur le prix du bien considéré dans le but de décider s'il est ou s'il n'est pas nécessaire de l'éliminer. Et après tout cela enfin il devra éliminer au moyen des procédés indiqués (dans les cas naturellement où leur application est possible) l'influence de chaque bien appartenant aux différents groupes, en laissant de côté seulement les biens dont l'influence ne soit pas, à son avis, considérable. Mais tout cela exige un travail énorme et souvent

compliqué et difficile. A cause donc de l'interdépendance des prix le statisticien, qui s'applique à ce genre de recherches, va se trouver dans l'alternative suivante: ou bien il n'en tient pas compte, et alors les résultats auxquels il peut parvenir ne seront que très largement approximatifs; ou bien il en tient compte dûment, et alors il est obligé de soumettre les différentes données aux procédés d'élimination indiqués. Mais dans ce cas il faut nous demander si les données qui vont nous rester après cette élaboration représenteront encore quelque chose de vivant, capable de nous dire ce que nous leur demandons, ou ne seront-elles plutôt, à cause des mutilations et des transformations subies, des branches mortes sans aucune signification. Nous nous bornons à poser la question, sans la résoudre. Aux statisticiens d'y répondre.

Université de Padoue

SOME CONSIDERATIONS ON THE ANALYSIS OF THE PRICES OF COMPETING OR SUBSTITUTE COMMODITIES

By Mordecai Ezekiel

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Investigators attempting to measure the forces responsible for the variations in the price of an individual commodity have almost always recognized that the supply and price of other commodities which might be used for the same purpose might influence both the price at which a given quantity of the commodity would be sold and the quantity of the commodity which would be purchased at a given price. Thus the production of corn has been considered as a factor in studying the price of oats;[1] the price of beef steers has been considered in studying the price of hogs;[2] hog prices have been considered in studying beef prices;[3] supplies of other fresh fruits have been examined in studying cantaloupe prices;[4] prices of hogs, steers, and veal, have been included in studying lamb prices;[5] supplies of sweet potatoes and corn meal have been considered in analyzing movements in rice prices;[6] and similar efforts have been made to recognize the influence of competing products in other studies of prices. In a recent brilliant paper, Bean and Thorne showed that changes in prices which previously had been ascribed to secular trends in demand were really related to changes in consumer purchasing power as indicated by the index of payrolls.[7] This analysis included the price of beef as a factor in explaining changes in

[1] Hugh B. Killough, "What Makes the Prices of Oats," *U. S. Department of Agriculture, Bulletin 1351*, (1925), p. 11.

[2] G. C. Haas and Mordecai Ezekiel, "Factors Affecting the Price of Hogs," *U. S. Department of Agriculture, Bulletin 1440*, (1926), pp. 35–38, 43–46.

[3] John A. Hopkins, Jr., "A Statistical Study of the Prices and Production of Beef Cattle," *Iowa Agricultural Experiment Station, Research Bulletin 101*, (1926), pp. 378–382.

[4] E. Rauchenstein, "Economic Aspects of the Cantaloupe Industry," *University of California Agricultural Experiment Station, Bulletin 419*, (1927), pp. 17–19, 36–37.

[5] Mordecai Ezekiel, "Factors Related to Lamb Prices," *Journal of Political Economy*, xxxv, No. 5 (April, 1927), pp. 251–259.

[6] Carlos E. Campbell, "Factors Affecting the Price of Rice," *U. S. Department of Agriculture Technical Bulletin 297*, (1932), p. 5.

[7] L. H. Bean and G. B. Thorne, "The Use of 'Trends in Residuals' in Constructing Demand Curves," *Journal of American Statistical Association*, xxvii (March, 1932), pp. 61–67.

pork prices, and the price of pork in accounting for variations in beef prices.

In spite of the general recognition of the importance of competing products, no one appears as yet to have attempted to formulate clearly the exact way in which they enter into the demand situation, or to determine on a logical basis which is the best way to bring them into the statistical investigation. It is evident from the previous discussion that sometimes the price of the competing product is used as the independent factor to be included in the statistical analysis, and sometimes the supply of the competing product, or the quantity consumed, is used instead. Is there any theoretical basis for preferring one or the other, or are there still other ways of treating the problem?

The general theories of Pareto, Walras, and Moore, [8] have stated the relation in its broadest sense, that the quantity of any product demanded is a joint function of the prices of all products. The neoclassical theory, as stated by Marshall, recognizes that the price of any one commodity is a function of the supply of all commodities. The economic statistician, however, has to narrow down the problem to the range within which the data available make it possible to secure results of statistical significance,[9] and to include only those factors which are of measurable significance.

Among the factors influencing the price of a given commodity, the supply of that commodity, the general level of prices, and/or the purchasing power in the hands of consumers, and the prices or supplies of competing commodities, would ordinarily be included. For the sake of clarity, the following analysis will exclude consideration of price level and purchasing power.

Using p and q for prices and x and y for supplies of two commodities, we may state the factors explaining p in two alternative forms:

$$p = h(x, y) \tag{I}$$

$$p = H(x, q) \tag{II}$$

h and H being two function signs. To simplify the statistical analysis these equations may be assumed of the form:

$$p = h_1(x) + h_2(y) \tag{III}$$

$$p = H_1(x) + H_2(q) \tag{IV}$$

[8] In *Synthetic Economics*, published in 1929, Henry L. Moore gives a condensed summary of Walras' theories.

[9] This requirement is developed in more detail in my review of Moore's *Synthetic Economics*, in the *Quarterly Journal of Economics*, XLIV, (August, 1930), pp. 676–677.

h_1, h_2, H_1 and H_2 being functions of one variable. Equation IV is the form which has generally been used in statistical analysis, as, for example, in the study by Bean and Thorne just cited.

The implications of equation IV may be further developed if we state it for both p and q.

Then

$$p = H_1(x) + H_2(q) \tag{IV}$$

$$q = K_1(p) + K_2(y). \tag{IV-a}$$

It is evident that in this statement we use the price q to explain changes in the price p, and conversely use the price p to explain changes in the price q. We may show more clearly what this involves by inserting in each equation the value for the independent price given by the other equation. Equation IV then becomes

$$p = H_1(x) + H_2[K_1(p) + K_2(y)] \tag{V}$$

and equation IV-a becomes

$$q = K_1[H_1(x) + H_2(q)] + K_2(y). \tag{V-a}$$

It is clearly evident from these last two equations that this form of analysis involves circular reasoning; i.e., that when the price p is explained by movements in the price q, it is in part explained by itself, since the movements in q are similarly dependent on p. Comparing equation (V) with equation (III), we see that the independent factors involved are the same in both, except that equation V also includes p as a factor in the explanation of the p; which is the mathematical formulation of the circular reasoning just pointed out.[10]

If we assume pork to be the first commodity, and beef the second, we may re-state this in more concrete terms. Suppose, in one year, some random event, such as an abnormally cool summer, had so stimulated demand as to cause prices of both products to be much higher than the other elements in the supply and demand situation would account for. If, then, prices of beef and pork were being analyzed on the basis of equations IV and IV-a, the unusually high price of pork would be explained as due to the high price of beef, and the unusually high price of beef would be explained as due to the high price of pork; and the fact that some additional random cause had been at work would be covered up in such a way that the investigator would be completely unaware of it.

If, however, the form of Equation III were used instead, the analysis for two commodities jointly would be of the form

[10] Equation (V) can be looked upon simply as an implicit definition of (III), or rather of (I). ED.

$$p = h_1(x) + h_2(y) \qquad \text{(III)}$$

$$q = k_1(x) + k_2(y). \qquad \text{(III-a)}$$

Circular reasoning of the type just discussed can enter here only where x is a function of p, or where y is a function of q. In most agricultural products this condition will not exist, for x during a given statistical period, such as a month or a year, will ordinarily depend little, if at all, upon p during that same period, and instead will be determined largely, or entirely, by p during the earlier periods when producers were doing their planting or breeding for subsequent production. In the case of hogs or beef, there can be some slight effect of current prices on the rate at which the animals are sent to market, or on the weight to which they are fattened; but if the supply for periods of a year as a whole are considered, these effects are small as compared to the effect of earlier prices which have influenced the numbers born and raised.

It is evident from this analysis that there is a marked danger of circular reasoning when the price of one competing product is used to explain the price of another, and that this danger is reduced, but not necessarily eliminated, when the supply of the competing product is used in the analysis rather than its price.

Empirical Test of the Theoretical Conclusions

These theoretical conclusions may be given an empirical test by trying to see how much difference, if any, there is between the results secured by the alternative methods in an actual case. The data used by Bean and Thorne in the study already referred to have been very kindly furnished me by them, and provided the basis for the following analysis.

The data (given as an appendix to this paper) covered the years 1922 to 1931. In working out the following correlations, only the data for the years 1922 to 1930, inclusive, or 9 observations, were employed, and the final year was used as an independent check on the usefulness of the observed relations for "forecasting" purposes. All of the data were stated in terms of logarithms,[11] giving five series as follows:

p = logarithm of the retail price of pork
x = logarithm of the consumption of pork
q = logarithm of the retail price of beef
y = logarithm of the consumption of beef and veal
D = logarithm of the index of payrolls.

[11] Logarithms were employed to include the effect of payrolls more accurately. A 10 per cent increase in payrolls, or a 25 per cent decrease, should presumably be reflected in a corresponding *proportional* change in the price of a given commodity, rather than an absolute change.

Four multiple correlations were run combining the variables as follows:

$$(1) \qquad p = H_1(x) + H_2(q) + H_3(D)$$

$$(2) \qquad q = K_1(p) + K_2(y) + K_3(D)$$

$$(3) \qquad p = h_1(x) + h_2(y) + h_3(D)$$

$$(4) \qquad q = k_1(x) + k_2(y) + k_3(D).$$

Correlations (1) and (2) paralleled those presented in the study by Bean and Thorne, and were of the same form as equation IV; correlations (3) and (4) used the supply of the competing product instead of its price, and thus were of the form of equation III.

The results secured may be summarized as follows:

Correlation number	Factors correlated	Observed multiple correlation		Error in estimating 1931 price
		$\bar{R}$	$\bar{P}$	
1	Pork price { Pork consumption, Beef price, Payrolls	0.947	0.994	per cent −11.5
2	Beef price { Beef consumption, Pork price, Payrolls	0.970	0.997	−15.7
3	Pork price { Pork consumption, Beef consumption, Payrolls	0.768	0.976	−18.3
4	Beef price { Beef consumption, Pork consumption, Payrolls	0.836	0.970	−15.8

The observed correlations are all high, even after adjusting for the number of degrees of freedom removed. There is a consistent difference however, between the results from two approaches, of the same order as would be expected from the theoretical analysis: correlations (1) and (2), where the price of the competing product is used as an independent factor show almost perfect correlation, definitely higher than shown by the other correlations. Subtracting $\bar{P}^2$ for the correlations where supplies alone were used for the independent factors (3) and (4) from $\bar{P}^2$ for the corresponding correlations (1) and (2), one might conclude that 3.6 per cent of the variance in pork prices was associated with variance in beef prices, but was not explained by differences in the consumption of either commodity or in payrolls. Similarly, 5.3 per

cent of the variance in beef prices was associated with variance in pork prices, and was independent of the other factors concerned. Although small, this additional variation which is unexplained in analyses (3) and (4) would be given a spurious explanation by (1) and (2); and would make the economic explanation appear far more complete than it really is.

The most interesting results are secured, however, on examining the residual or unexplained variation in each series of observations. This may be done by working out estimated prices according to each set of correlations, subtracting estimated price from the actual price, and thus determining the unexplained residual for each individual observation. These residuals, or "errors of estimate," that is to say the departures of the actual pork and beef prices from the estimated prices, were generally in the same direction when the estimate was based solely on consumption of both products and on the index of payrolls. Some common factor, or factors, affected both prices throughout the period, in addition to these three economic elements. When the estimated price was based partly upon the price of the competing product, however, there was little relation between the departures in the two series—the correlation process itself had automatically eliminated the influence of any other factor affecting both prices in common, without leaving any clue as to what it really might be, and had thus given a spuriously high measure of the completeness with which factors determining price had been isolated. The computed correlation between these two series of residuals confirms these visual conclusions: for the first series, the correlation is 0.75; for the second, it is but 0.31. According to R. A. Fisher's criterion as to the significance of correlation coefficients obtained from a small sample,[12] the chances are better than 19 out of 20 that the observed correlation between the residuals in the first series could not have occurred by chance and that the true correlation is at least 0.17; whereas the low correlation in the second series might readily have occurred by chance. These results are in agreement with the theoretical expectations.

Turning to the slope of the several regression lines, the results compared as tabulated on the following page.

It is noticeable, both with consumption and payrolls, that the observed regressions were much smaller when price of the competing product was used as an independent factor than when the alternative method was followed. In this case the use of the price of the competing product not only resulted in a spuriously high multiple correlation, but

[12] See graphic chart in "*Methods of Correlation Analysis*," p. 393, by Mordecai Ezekiel, for size of correlation required for significance for coefficients computed from small samples of various sizes, according to results of R. A. Fisher.

over-emphasized the influence of the competing product on the price, and thereby resulted in an erroneously low measure of the effect of changes in each of the other factors upon the price of the product. The use of the price of the competing product as a factor in the correlation apparently tends to distort and invalidate all the conclusions reached by the statistical analysis.

	Treatment of competing product in correlation	
	Price used	Consumption used
Regression of logarithm of price on logarithm of supply		
Pork prices	−1.143	−1.441
Beef prices	−1.843	−2.388
Regression of logarithm of price on logarithm of payrolls		
Pork prices	+ .635	+1.218
Beef prices	+ .631	+1.574

Further Comments

In the illustration just examined, there was found to be a significant difference between the results secured by the two methods of stating the competing product, and the statement using supply was found to give more valid results. This illustration, however, satisfied fairly well the condition that the supply of the competing product during the period under investigation was not a function of the price of the same product. In other products, where supply is more quickly responsive to price, this condition might not be fulfilled, and so even the second method might be unsatisfactory. This is, however, merely a special aspect of the general unsettled question as to the proper methods of price analysis for products where supply may be readily adjusted to price,[13] and need not be gone into further at this point.

The real difficulty arises from the fact that our methods of statistical analysis require that we take only a single factor as the dependent factor, and consider all other factors as independent factors; where-

[13] For the elements in this controversy, see Elmer Working, "What do Statistical 'Demand Curves' Show?" *Quarterly Journal of Economics*, (Feb., 1927); Mordecai Ezekiel, "Statistical Analysis and the 'Laws' of Price," *Quarterly Journal of Economics*, XLII, (February, 1928), p. 199; Wassily Leontief, "Ein Versuch zur Statistischen Analyse von Angebot und Nachfrage," *Weltwirtschaftliches Archiv*, Band XXX, Heft 1, (July, 1929), pp. 1–53; Elizabeth Waterman Gilboy, "Demand Curves in Theory and in Practice," *Quarterly Journal of Economics*, XLIV, (August, 1930), p. 601.

as in many cases the values are actually neither dependent nor independent, but are determined by some sort of mutual inter-dependence. In such cases p, q, x, and y, may be thought of as mutually balanced in a general equilibrium. Four balls, finding their position in the bottom of a hemispherical bowl, might be taken as a physical example of such interaction, but not as a perfect illustration. No statistical method has yet been evolved for analyzing such mutual equilibriums, and not even a clearcut theoretical analysis, in mathematical terms, has yet been developed.

Certain partial approaches to the general problem may be offered. Thus it is possible that, in the particular case used for an illustration, the *average* price paid for a given consumption of meat might be determined by the total consumption, regardless of its composition, and that the *relative* prices for beef and pork might reflect the relative supplies of each. Such a situation might be expressed mathematically as follows:

$$P = \frac{xp + yq}{x + y} = \text{weighted average price of beef and pork} \qquad \text{(VI)}$$

Then we may assume

$$P = F(x + y) \qquad \text{(VII)}$$

and further assume

$$\frac{p}{q} = \Psi\left(\frac{x}{y}\right). \qquad \text{(VIII)}$$

In the case of the data used in the illustration, there is actually a very close correlation of the type indicated in (VIII).

The magnitudes in equations (VI), (VII), and (VIII) may be so substituted and re-written as to give a single equation in which only p, q, x, and y, are involved,[14] and which states the general equilibrium; but this equation is so involved that it is not in such form that the values of the various parameters can be determined by ordinary statistical procedures.

The problem of analyzing the prices of two competing or substitute products may be further generalized to include 3, 4, or n such products. Obviously, as the number of commodities considered is increased, the possibility of reducing the analysis to dimensions which are amenable to statistical investigation is reduced. Following through this aspect of the problem would lead one away from the factors responsible for

[14] This equation is not uniquely determined. By the assumptions stated, there may be written out an infinity of equations involving p, q, x and y. ED.

variations in a single selected price to factors responsible for variations in a number of related prices, and in their inter-relationships, and eventually into factors reponsible for variations in all prices; and thus into a far wider field of investigation than statistical price analysis has as yet dared to tackle.

Conclusion

The problem of the most effective way to measure the influence of a competing product when analyzing the price of a given product is far from complete solution. If the price of the competing product is used as an independent factor in the price analysis there will be danger of spurious correlation, to the extent that the price of the competing product is itself a function of the price of the given product. If the supply of the competing product is used as the independent factor, the possibility of spurious correlation is eliminated under certain conditions, or is minimized under others. These comments apply to the case where the relationship of two competing products is investigated by making a separate price analysis for each product. It is possible that methods might be developed which would determine directly the mutual relationship of competing products, with supplies and prices of both considered simultaneously; but not even a start in this direction has yet been made.

Federal Farm Board

Appendix: Data used in Correlation Analysis

Year	Composite retail prices[a] (Cents per pound)		Total consumption of federally inspected (Billion pounds)		Index of payrolls lagged 3 months
	Beef	Pork	Beef & Veal	Pork	
1922	24.1	34.7	4.94	5.78	76.3
1923	24.9	33.2	5.13	7.06	100.2
1924	25.3	33.2	5.28	7.20	97.4
1925	26.1	39.8	5.53	6.30	97.9
1926	27.0	42.3	5.74	6.15	105.6
1927	28.2	39.5	5.36	6.71	102.8
1928	32.4	37.5	4.81	7.33	100.1
1929	34.5	38.4	4.85	7.30	107.3
1930	31.6	37.1	4.77	6.92	93.4
1931	25.9	31.1	4.78	7.06	70.2

[a] Average of monthly prices weighted by consumption.

ON THE DEMAND FOR RIVAL (OR SUBSTITUTE) COMMODITIES

By Umberto Ricci

Paper read by title at the meeting of the Econometric Society, New Orleans, January, 1932

1. A commodity may have many *substitute* or *rival* commodities, and sometimes a large series of them, if we include among substitutes also the different qualities of the same good. Let us consider, for instance, a suit of clothes. There is a whole range of suits for any one person, owing to differences in cloth, the cleverness of the tailor, his renown, and so on. One may range from a suit bought ready-made in a shop to a smart suit prepared after many fittings by a brilliant specialist. Very seldom does a person have an unlimited freedom of choice, owing to differences of education, of taste, and, above all, of social class. A worker, even if he suddenly becomes wealthy, will feel himself prevented from having access to a high-class tailor, and, vice versa, a man of some social distinction will not buy standardized clothes. However, one may always choose among a certain number of rival commodities.

2. Let us begin by supposing that the commodities are all *indivisible*. Then, if one single item is sufficient for the full satisfaction of the want in the unit of time, the price coincides with the expense of the individual budget on that particular chapter. Let us range all the prices (or expenses) of a set of rival goods in increasing order of magnitude:

$$s_1, s_2, s_3, \cdots . \tag{1}$$

The utilities of the corresponding goods are denoted:

$$U_1, U_2, U_3, \cdots . \tag{2}$$

Let us calculate the quotients

$$u_1 = \frac{U_1}{s_1}, \quad u_2 = \frac{U_2}{s_2}, \quad u_3 = \frac{U_3}{s_3} \cdots . \tag{3}$$

They represent evidently the *average* utility of one unit of money spent in these various directions. May we detect some regularity in the series of those quotients? I think that, when running along the series (1) and (2), we will generally note that the utilities U are increasing, but in the beginning more, and later less, than in proportion to the corresponding s. In other words, the series of the quotients will be first increasing and then decreasing.

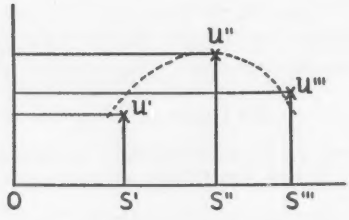

Fig. 1

If along a horizontal axis we measure the expenses, so that (Fig. 1)

$$\overline{OS'} = s_1, \quad \overline{OS''} = s_2, \cdots$$

and on the vertical axis we measure the quotients, so that (Fig. 1)

$$\overline{U'S'} = u_1, \quad \overline{U''S''} = u_2,$$

the rectangles such as OU' will represent the utilities of the various rival goods. Certainly the points U will not be so regularly disposed as to permit fitting to them a curve like the dotted line in Fig. 1. But, for the possibility of using mathematical analysis, at least in a first stage, we will assume that a continuous curve, having the equation

$$u = u(s) \tag{4}$$

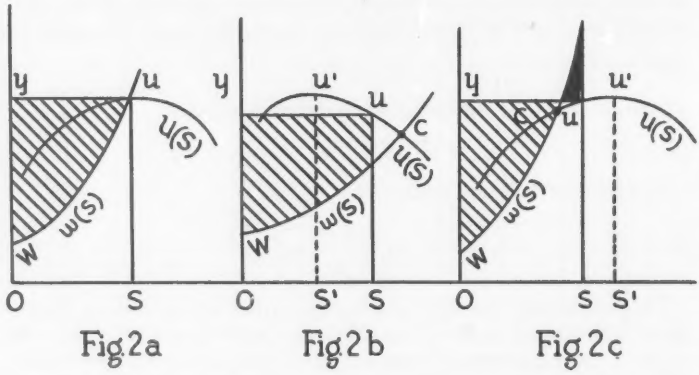

Fig 2a Fig 2b Fig 2c

represents the *curve of average rival utilities* (see for instance Fig. 2). Take a point U on such a curve: its abscissa is the price of one item, and the rectangle of its coordinates represents the utility of this item.

3. We want another curve, having the equation

$$w = w(s) \tag{5}$$

which we call the *curve of marginal sacrifices* incurred for acquiring one of the rival goods. Taking a point U on such a curve (see Fig. 2a), the abscissa OS is the sum to be spent for buying one of the rival goods, and the area $WOSU$ or the integral

$$\int_0^s w(s)ds$$

is the total utility which the person in question derives from the last s units of his money, if he spends them (according to the urgency of his wants) in buying anything *except any one* of the rival goods.

4. To every point of the curve of average utilities there corresponds a psychic rent R, which is equal to the utility of the particular good less its psychic cost, or sacrifice. That point (or good) will be chosen which affords the greater rent. Now since

$$R = su(s) - \int_0^s w(s)ds = R(s) \tag{6}$$

the expense s_μ which maximizes the rent is obtained by solving the equation

$$u(s) + su'(s) - w(s) = 0$$

or

$$\frac{u(s) - w(s)}{s} = -u'(s). \tag{7}$$

And of course the maximum rent is

$$R_\mu = R(s_\mu). \tag{8}$$

The average utility point to which the maximum rent corresponds (if this rent is a positive quantity) is the point of the consumer's equilibrium. Its abscissa is s_μ.

5. Let us call s_c the abscissa of the point C (Fig. 2b and 2c) where the curve of average utilities cuts the curve of marginal sacrifices, so that we have by definition

$$u(s_c) - w(s_c) = 0$$

and s_m the abscissa of the point U', where the curve of average utilities reaches its maximum, so that we have by definition

$$u'(s_m) = 0.$$

Then (7) shows that if $s_c = s_m$ it follows that

$$s_c = s_\mu = s_m.$$

If, on the contrary, $s_m < s_c$ (as in Fig. 2b), the maximum point for the rent must be to the right of s_m and to the left of s_c, so that we have

$$s_m < s_\mu < s_c.$$

If, finally, $s_m > s_c$ (as in Fig. 2c) it follows that

$$s_c < s_\mu < s_m.$$

These propositions may be graphically illustrated thus:

(1) If the two curves of average utilities and of marginal sacrifices are mutually intersecting at the point U (Fig. 2a) where the first curve reaches its maximum, then U is the equilibrium point and the maximum rent is the triangle YWU.

(2) If the intersection of the two curves $u(s)$ and $w(s)$ occurs to the right of the point U' where the average utility is a maximum, (as in Fig. 2b) the point of equilibrium U is between U' and C. The shaded area is the rent.

(3) If, finally, the intersection of the curves $u(s)$ and $w(s)$ occurs at the left of the point U', (as in Fig. 2c) the point of equilibrium, as in the previous case, is between U' and C, but the rent is the shaded area less the black triangle.

6. Having fixed a certain magnitude for R, we may construct an average utility curve whose property is that at any of its points the rent afforded is constant and equal to R. Such a curve we may call an *indifference curve*. Its shape is dependent only on the shape of the curve of marginal sacrifices $w(s)$ and on the magnitude of R. Given (5), it is sufficient to integrate the differential equation:

$$dR \equiv s\,du + \{u(s) - w(s)\}\,ds = 0 \tag{9}$$

to obtain the equation

$$u = f(s, R) = f(s) \tag{10}$$

of the indifference curves. We get one such indifference curve for every magnitude of R. This magnitude we shall call the *index* of the particular indifference curve.

Suppose a horizontal plane covered with indifference curves for various magnitudes of the parameter R. The equilibrium point is reached when the curve of average utilities $u(s)$ touches an indifference curve, and the index of that indifference curve is the maximum rent.

It is easy to show that every indifference curve reaches its minimum when it cuts the curve of marginal sacrifices.

Indeed, let I (Fig. 3a) be a point common to the indifference curve $f(s)$ and the curve of marginal sacrifices $w(s)$. The index of the indifference curve is the area YWI. Another point I' of the same indifference curve, if placed at the left of I, must necessarily be higher than I. Since the areas $Y'WW'I'$ and YWI are equal by definition, the shaded rectangle and triangle must consequently be equal.

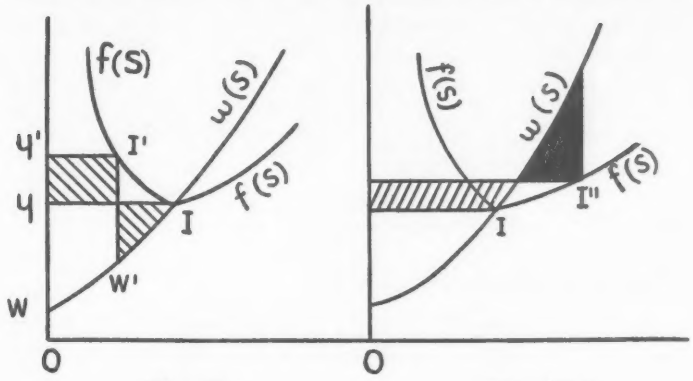

Fig. 3a Fig. 3b

If the other point I'' is to the right of I it must also be higher. And the shaded and the black areas of Fig. 3b must be equal and on opposite sides of the horizontal line passing through I''.

7. Let us give a numerical example just to make more concrete the conclusions reached in the previous paragraphs. Let the curve of marginal sacrifices have the equation:

$$w(s) = 1 + s. \tag{11}$$

It is difficult to imagine a more simple curve than this. As the equation of average utilities curve we take

$$u(s) = n + 2s - \frac{s^2}{2}.$$

This defines a family of parabolas of the second degree. Such parabolas, always preserving the same size and form, are shifted up and down according as the parameter n increases or decreases.

The abscissa which maximizes the average utility u is

$$s_m = 2.$$

The abscissa of the point of intersection between the two curves $u(s)$ and $w(s)$ is[1]

$$s_c = 1 + \sqrt{2n - 1} \ . \tag{12}$$

It coincides with $s_m = 2$ only when $n = 1$. Therefore if $n > 1$ the equilibrium point is on the decreasing arc of the average utility curve, and if $n < 1$ it is on its increasing arc.

The rent is

$$R = s(n - 1) + \frac{3}{2}s^2 - \frac{s^3}{2} \cdot$$

Its maximum is reached when

$$s = s_\mu = \frac{3 + \sqrt{3(2n + 1)}}{3} \cdot \tag{13}$$

The corresponding magnitude of R is

$$R_\mu = \frac{9n + (2n + 1)\sqrt{3(2n + 1)}}{9} \cdot \tag{14}$$

If, instead of fixing n and deducting R_μ, we fix R_μ and wish to find the corresponding n, we must solve

$$8n^3 - 15n^2 + 6n(9R_\mu + 1) - (27R_\mu^2 - 1) = 0.$$

According to (9), the differential equation of the indifference curves is

$$s\,du + (u - 1 - s)ds = 0.$$

Integrating it, we obtain

$$u = f(s, R) = 1 + \frac{s}{2} + \frac{R}{s} \cdot \tag{15}$$

This is the equation of the family of indifference curves generated by the curve of sacrifices (11). The curves (15) are all asymptotic to the vertical axes. They reach their minimum when

$$s = \sqrt{2R} \tag{16}$$

Substituting this value for s both in (15) and (11) we obtain in any case

[1] We have discarded the minus sign, because it could be taken into account only when n is between 1/2 and 1, and even in that case the corresponding rent would not be a maximum.

(17) $$u = 1 + \sqrt{2R}\,.$$

This confirms that the points of minimum are all on the curve of marginal sacrifices.

In order to find the abscissa of the point where the curve of average utilities and the indifference curve touch each other, we must solve the equation

$$\frac{df(s)}{ds} = \frac{u(s)}{ds}$$

or

(18) $$2s^3 - 3s^2 - 2R = 0\,.$$

8. Fig. 4 corresponds to the hypothesis

$$n = 5,5\,.$$

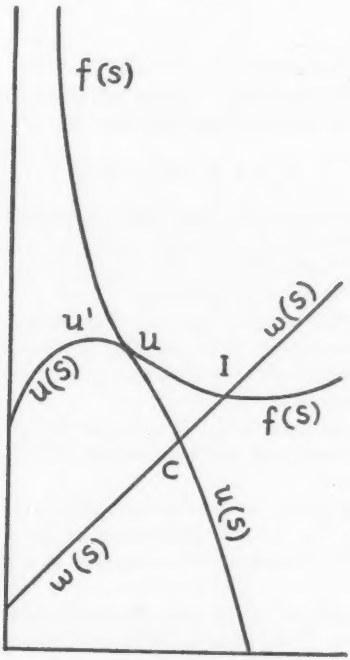

Fig. 4

From (13) and (14) we deduce

$$s_\mu = 3 \qquad R_\mu = 13,5$$

and from (15)

$$f = 1 + \frac{s}{2} + \frac{13,5}{s}.$$

It is represented by the $f(s)$ curve of Fig. 4. Its minimum point, according to (16) and (17), has the coordinates:

$$s = \sqrt{27} \cong 5,20 \qquad f \cong 6,20.$$

According to (18) we have the equation

$$2s^3 - 3s^2 - 27 = 0$$

from which

$$s = 3.$$

It is the same value already found from (9).

According to (12), the point C, where the utility curve $u(s)$ and the curve of sacrifices $w(s)$ are cutting each other, has the abscissa

$$s_c = 1 + \sqrt{10} \cong 4,16.$$

9. Let us now revert to the hypothesis, more akin to reality, that we only possess some points of average utilities, however scattered on the plane. To every point corresponds a rent. That point will be chosen to which the maximum rent corresponds.

10. We may further imagine that the rival goods are *divisible*. Then every *point* of *average* utility is replaced by a *curve* of *marginal* utility and, instead of a single curve of average utilities, we have a group of curves of marginal utility.

Every curve of marginal utility is to be treated separately, in order to find the equilibrium, and the equilibrium affording the maximum rent is to be preferred.

If, instead of a divisible, we have an indivisible good, but more than one item is desired for the full satisfaction of the want, then every point of the curve of average utility is replaced by a graph in the form of steps.

11. If the *price* of one of the rival goods increases, the other will probably follow, but, in the interval, it is possible that the good whose price has first increased, will be discarded. If the good is indivisible, and one item is sufficient, the corresponding point of the curve of average utilities will be shifted along a rectangular hyperbola, and a

new point must be substituted for the old in order to calculate the new rent. If, on the contrary, more than one item of the indivisible good is wanted, or the good is divisible, the diagram of utility—that is, the curve in the form of steps, or the curve of marginal utility—must be *transformed*. The transformation is effected by multiplying the abscissae and dividing the ordinates of the original diagram by the ratio of the change of the price.

Faculty of Law, Giza, Egypt

LES TROIS NOTIONS DE L'ÉQUILIBRE ÉCONOMIQUE[1]

PAR G. H. BOUSQUET

Communication présentée à la réunion de la Société d'Économétrie, Lausanne, septembre 1931

LA NOTION d'équilibre économique, qui a été fournie à la science grâce aux immortelles études de Walras et de Pareto, a peut-être été trop monopolisée jusqu'ici par l'économie mathématique, tandis que l'économie littéraire l'a négligée, à tort selon nous, parce qu'elle voyait en elle une subtile notion mathématique sans rapports avec les faits concrets. Dans la présente étude, nous voudrions nous placer à un point de vue tout différent: critiquer bien plutôt la notion d'équilibre mathématique, et montrer tout l'intérêt d'une notion plus directement littéraire.

Ce ne sont donc pas seulement des subtilités mathématiques que la théorie nous enseigne ou plus exactement devrait nous enseigner, c'est quelque chose de plus général, et qui se trouve renfermé dans la notion d'équilibre. Quels services cette notion est-elle susceptible de nous rendre?

Elle peut être envisagée sous trois aspects de plus en plus généraux, de plus en plus débarrassés du formalisme mathématique, en conséquence de plus en plus instructifs: (a) On peut d'abord s'attacher à l'idée d'équilibre mathématique statique; (b) On peut ensuite déduire de l'existence de cet équilibre l'application possible de la loi de Le Chatelier. (c) On peut enfin, puisqu'il y a mutuelle dépendance, en déduire qu'il ne faut pas se borner à l'étude de phénomènes particuliers, mais voir aussi dans quelle mesure ils se rattachent à l'ensemble des phénomènes économiques, puisque l'étude des phénomènes isolés a précisément pour effet de faire négliger cette mutuelle dépendance, cette vue d'ensemble de la question.

A. LE PREMIER ASPECT

La notion d'équilibre mathématique statique, tel que le conçoivent Walras et Pareto, est utile, mais c'est un instrument de recherche, *non une image des faits contemporains.* Il est bon de s'en pénétrer, car, en partie, notre monde économique est statique, mais en partie seulement: il s'y trouve des éléments qui ne varient pas trop vite. Il est

[1] Certains points de la présente communication se retrouvent plus développés, d'autres beaucoup moins, dans nos *Institutes de Science Économique* T.1. (Giard, éd.)

meilleur encore de l'étudier comme exercice pour l'esprit, en tant que discipline intellectuelle.

Lorsque nous examinons ensuite les faits, nous pouvons mesurer en quoi ils diffèrent du schéma statique, cela nous permet de les comprendre mieux que si l'on en abordait directement l'étude. Le concept de la statique économique est clair, net, ferme, il se prête admirablement au traitement mathématique. Il est donc susceptible de nous rendre des services, tant que nous en restons maître, tant que nous ne nous laissons pas dominer par lui. Mais vouloir raisonner sur les faits concrets, comme s'il s'agissait d'un équilibre statique, est très dangereux et tout à fait déraisonnable.

De plus, même si l'on ne tient pas compte de ce que la société contemporaine est dynamique, nous trouvons dans les équations de l'équilibre mathématique des imprécisions et des insuffisances sur lesquelles on n'a pas assez attiré l'attention:

1°. Chez Walras, par exemple, il n'est absolument rien dit sur la nature des fonctions d'ophélimité (en particulier le degré de ces fonctions). Dans ces conditions, sans doute, au point de vue mathématique, "le problème est déterminé" car il y a autant d'équations que d'inconnues, mais au point de vue de la réalité concrète, il ne l'est pas, car le système peut avoir plusieurs racines. En d'autres termes à partir des données de Walras (goûts et quantités des forces productrices), plusieurs positions d'équilibre stable existent peut-être. On ne trouve pas ces observations chez les économistes mathématiques, dont l'affirmation, "le problème déterminé" n'a donc pas grande application directe aux faits.

2°. Il y a autant d'équations que d'inconnues. Sans doute, mais seulement parce que l'économie mathématique a éliminé les conditions concrètes qui peuvent la gêner dans un traitement abstrait de la question. Et pourtant parmi celles-ci il y en a une au moins, des plus générales, précisément découverte par V. Pareto: savoir la forme de la courbe des revenus qui ne figure pas dans ses propres équations. Si cette condition n'est pas ajoutée, la valeur de la théorie mathématique comme image de la réalité concrète s'affaiblit considérablement. Si on l'ajoute, on a un système qui a plus d'équations que d'inconnues.[2]

[2] La courbe des revenus n'est pas une *équation* indépendante mais une *conséquence* des autres *données* de la théorie de l'équilibre de Pareto (de la nature des goûts, des conditions initiales, etc.). Donc il n'y a pas plus d'équations que d'inconnues. Ed.

Pareto se base sur des statistiques et pas du tout sur la déduction. Il est impossible de déduire de la *théorie* de l'équilibre le *fait concret* de la courbe des revenus.
G. H. B.

Il ne faut pas confondre: (1) la courbe des revenus en tant que loi théorique (sous cette forme elle peut être déduite des autres données) avec (2) la courbe des

3°. Par une foule d'autres points, la théorie mathématique abstraite est une image fort altérée des faits. Je n'en veux pour preuve que le fait suivant (relevé d'ailleurs par Zawadzki): dans le bilan de l'entrepreneur doit figurer la condition qu'en moyenne, il ne fait ni pertes ni bénéfices, et aussi que la dernière unité produite par lui, couvre juste les frais de production. Ces deux conditions seront le plus souvent incompatibles.

En résumé, il ne faut pas s'absorber exclusivement dans la méditation de cette notion abstraite: l'équilibre mathématique statique général. Cela risque d'être préjudiciable à l'avancement de la science.

B. Le second aspect

Nous pouvons interpréter la théorie de l'équilibre dans le sens statique. Nous pouvons également l'interpréter dans le sens suivant: le système économique doit être le siège de réactions qui obéissent la loi de Le Chatelier. Ici nous abandonnons la lettre des enseignements de Walras et de Pareto et nous les interprétons d'une façon plus générale et plus instructive.

D'après la loi de Le Chatelier, "la modification d'une quelconque des conditions pouvant influer sur l'état d'équilibre d'un système provoque une réaction dans un sens tel qu'elle tend à amener une variation de sens contraire à la condition extérieure modifiée."

Or la théorie de l'équilibre économique montre qu'à certains égards un système économique ressemble à un système physico-chimique. Rien d'étonnant, par conséquent, lorsqu'on examine des parties de ce système d'observer des réactions qui suivent la loi de Le Chatelier. Nous trouvons dans une considération d'ensemble du système l'explication de certains faits particuliers.

Voici quelques exemples: lorsque, dans un état de libre concurrence, la demande augmente (trouble à l'équilibre), le prix de vente hausse (effet de ce trouble sur le système), mais cette hausse du prix de vente tend à produire une augmentation de la production qui amène comme conséquence une baisse dudit prix de vente (tendance au rétablissement de l'équilibre). Ce fait était déjà bien exprimé par Dupont de Nemours à propos du prix du blé lorsqu'il disait: "Ce que l'on appelle cherté, c'est l'unique remède à la cherté."

Entre deux pays à étalon or, la hausse du cours des changes entraîne une sortie d'or, qui tend à faire baisser ce cours.

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revenus comme fait empirique (sous cette forme elle n'entre pas du tout dans le schéma théorique, mais peut servir seulement à comparer les résultats théoriques avec l'observation). Dans l'un et l'autre cas le nombre d'équations indépendantes reste le même, indépendamment du fait que la courbe des revenus est "ajoutée." Ed.

Lorsque la production du métal étalon augmente, la quantité de monnaie tend à augmenter; cette augmentation fait hausser les prix et rend plus difficile la production du métal-étalon.

Ces phénomènes sont analogues aux suivants: "Une élévation de température provoque une réaction tendant à produire un abaissement de température, c'est à dire une réaction avec absorption de chaleur. Une augmentation de pression produit une réaction tendant à amener une diminution de la pression, c'est-à-dire une réaction avec diminution de volume, . . . l'augmentation dans un système homogène de la masse des corps en équilibre provoque une réaction tendant à diminuer la masse de ce même corps" (H. Le Chatelier, *Leçons sur le carbone*, p. 357).

Interprétée de cette façon la théorie de l'équilibre devient remarquablement suggestive. Mais il faut, comme tout à l'heure, savoir l'utiliser, et ne pas en devenir l'esclave: comme le monde économique concret ressemble partiellement à un système en équilibre, nous avons l'occasion d'y observer des réactions selon la loi de Le Chatelier, et nous comprenons pourquoi. Grave, néanmoins, serait l'erreur de croire à une application continuelle et nécessaire de cette loi, car les prémisses de la théorie, diffèrent de la réalité. Aussi bien d'ailleurs, la chimie physique connait-elle des équilibres instables, où le moindre trouble apporte une modification profonde du système qui ne tend pas du tout à rétablir l'ancien état de chose. Il en va de même en économie politique. Par exemple, la construction d'un chemin de fer ne fait pas du tout naître des forces qui tendent à sa suppression. Bien souvent, elle permet d'activer tellement les échanges que la pose d'une nouvelle voie devient nécessaire. Lorsque l'inflation atteint un certain degré d'acuité, il naît dans le système des réactions qui rendent désirable, et même nécessaire, l'impression de nouveaux billets de banque. Dans tous ces cas, l'on se trouve en présence d'équilibres instables, *et seule l'expérience peut se prononcer à ce sujet.*

C. Le troisième aspect

En dernier lieu on peut interpréter l'équilibre économique comme étant une théorie de la mutuelle dépendance générale des éléments du système, que ce système soit ou non statique, que s'y vérifie ou non, la loi de Le Chatelier. C'est là le sens le plus général, le plus profond, le plus instructif de ce concept.

Il s'en déduit une conséquence très importante: tous les éléments de la vie économique sont liés entre eux.[3]

[3] Nous croyons devoir signaler ici une vague analogie, avec la "loi de la corrélation des formes" de Cuvier (*Recherches sur les ossements fossiles de quadrupèdes, etc.* [Paris, 1812], tome 1, p. 58): "Tout être organisé forme un ensemble, un système unique et clos dont toutes les parties se correspondent et concourent à

Si l'on examine un fait économique *isolé* on arrive à d'autres conclusions touchant sa nature que si l'on examine *un grand nombre* de cas semblables.

Pourquoi en est-il ainsi? La théorie moderne de la science permet, semble-t-il, de répondre à cette question. Les théories scientifiques ne sont pas absolument vraies, elles sont seulement vraies approximativement.

Il y a des cas où la différence entre l'affirmation théorique et la réalité est si faible qu'elle est pratiquement négligeable. On rencontre beaucoup de cas semblables lorsqu'on étudie des faits économiques isolés. Mais lorsqu'on veut étendre les conclusions de cette étude à de nombreux faits semblables, l'erreur d'approximation devient de plus en plus sensible et finit par rendre entièrement fausse la conclusion.[4]

Dans ces conditions, la prise en considération de l'équilibre économique s'oppose nettement à l'examen partiel d'un cas ou d'un petit nombre de cas pour lesquels on néglige la mutuelle dépendance. Aussi peut-on, nous semble-t-il énoncer ici une *loi du petit nombre:*

"*Certaines considérations économiques ne sont vraies que pour un petit nombre de phénomènes semblables,* elles cessent d'être vraies lorsque ce nombre augmente."

Pour exposer clairement la chose, commençons par prendre des exemples en dehors de l'économie politique. Il y a quelques années, le *Petit Parisien* publiait dans un de ses numéros le conte suivant: pour récompenser un vieux facteur des postes, les habitants d'un village décident de lui offrir un tonneau de vin, chacun devant verser un litre dans le tonneau. Malheureusement, le tonneau plein, le pauvre homme n'y trouve que de l'eau; chacun avait compté sur son voisin. Or il est bien vrai que l'avarice d'un ou deux villageois ne lui eut causé en somme aucun tort, mais cela n'est plus vrai pour un grand nombre et encore bien moins pour leur ensemble.

On croirait en vérité que cette histoire fut connue de Thucydide,

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la même action définitive par une action réciproque. *Aucune de ces parties ne peut changer sans que les autres changent aussi,* et par conséquent [?] chacune d'elles, prise séparément, indique et donne toutes les autres." On sait que cette conclusion sous sa forme extrême a été justement combattue, mais elle n'est pas entièrement inexacte, et elle se vérifie aussi partiellement dans le monde social. Dans l'équilibre économique, il est vrai, l'individu, ou un groupe restreint d'individus, joue un rôle assez semblable à celui d'une cellule dans des organismes pluricellulaires ou l'unité fonctionelle ne serait pas parfaite, puisque l'on a vu des cas (Robinson Crusoe) où ceux-ci arrivent à vivre en dehors de la société.

[4] Voir dans E. Borel, *Le Hasard,* p. 39 s., un très bel exemple d'erreur de raisonnement provenant de ce que l'on admet comme absolument exact, ce qui ne l'est qu'à peu près, et que l'on oublie ensuite cette restriction au moment où l'erreur est de nature à altérer le résultat.

lorsqu'il appliquait ainsi la loi du petit nombre: (1.114) "Aucun ne croit que sa négligence particulière fasse tort au bien général. Il pense qu'un autre y pourvoira pour lui et tous séparément ayant la même pensée, l'intérêt commun se détruit sans qu'on s'en aperçoive."

Voici maintenant des exemples économiques: Il est très vrai que la découverte de la pierre philosophale enrichirait son inventeur et un certain nombre de fabricants d'or. Il est tout à fait faux que si tout le monde fabriquait de l'or, il en résulterait un enrichissement quelconque de la société, cela serait au contraire une catastrophe pour elle.

Laissons la parole à Malthus (*Princ. de pop.* IV, trad. franç., chap. 33, n.) "Dans l'état actuel lorsqu'un pauvre cultivateur perd une vache, on y porte remède par une pétition et une souscription en sa faveur.... Mais si le système des vaches devenait universel, les pertes de cette nature seraient si fréquentes qu'on ne pourrait plus les réparer par de tels moyens."

A. Lansburgh (*Die Kreditbank*) fait remarquer que notre système bancaire constitue l'organisation de l'illiquidité. Il existe sans doute des procédés qui permettent à *une* banque à un moment donné de rendre sa situation plus liquide, mais cela n'est plus vrai de leur ensemble. Car lesdits procédés ont pour conséquence de rendre la situation des autres banques plus difficile.

Marx observe (*Das Kapital*, tome III, p. 363):

Der einzelne Kapitalist hat die Wahl ob er sein Kapital; . . . als zinstragendes Kapital oder als produktives Kapital selbst verwerthen will. Allgemein gefasst, d.h. auf das ganze Gesellschafts Kapital angewendet, wie dies von einigen Vulgär ökonomen sogar als Grund des Profits angegeben wird, ist dies natürlich verrückt. Die Verwandlung des sämtlichen Kapitals in Geldkapital, ohne dass es Leute gibt die die Produktionmittel kaufen und verwerthen, . . . dies ist natürlich Unsinn."

Dans *Ce qu'on voit et ce qu'on ne voit pas*, le bon Bastiat fait des applications souvent heureuses de notre principe qui s'applique également en matière démographique:

Chez nous [aux Pays-Bas], les chances de vie moyenne d'un nouveau-né mâle lui permettent d'atteindre 46,2 ans. Mais on n'en saurait déduire pour *un* enfant determiné qu'il mourra précisément à cet âge. Ce n'est que pour *un groupe* suffisamment grand d'individus que l'on peut dire qu'ils atteindront en moyenne cet âge.... (A. Verryn Stuart, *Inleid. tot de beoefening des Statist.* Haarlem, 1910.)

Mais la théorie de l'équilibre interprétée comme une théorie de la mutuelle dépendance peut nous rendre encore d'autres services, même si pris en masse les phénomènes observés restent semblables au phénomène isolé: (1) La théorie nous apprend à ne jamais perdre de vue la mutuelle dépendance *économique* des phénomènes, c'est-à-dire

à examiner quelle répercussion une variation d'un des éléments peut avoir sur tous les autres (et pas seulement sur les éléments semblables); (2) La théorie nous suggère d'examiner s'il n'existe pas, englobant la mutuelle dépendance des phénomènes économiques, une mutuelle dépendance des faits sociaux en général, notion des plus fécondes ainsi que l'a montré V. Pareto en sa *Sociologie*. Ainsi, même lorsqu'il n'en sera pas explicitement question, il ne faut pas oublier que les phénomènes économiques dont nous traiterons se trouvent liés à d'autres phénomènes sociaux, et qu'ils réagissent tous les uns sur les autres.

En résumé, l'idée d'équilibre économique convenablement interprétée nous apparaît à tous égards comme des plus suggestives. On ne saurait assez s'en pénétrer lorsqu'on aborde les études économiques concrètes.

L'œuvre de Walras et de Pareto est pleine d'enseignements les plus féconds, je dirai; *surtout* pour l'économie littéraire.

Université d'Alger

COLLEGIATE MATHEMATICS NEEDED IN THE SOCIAL SCIENCES

A report prepared for the Social Science Research Council by a Committee consisting of H. R. Tolley of the University of California (Chairman), F. L. Griffin of Reed College, Holbrook Working of Leland Stanford University, Charles H. Titus of the University of California at Los Angeles, and Mordecai Ezekiel of the Federal Farm Board, and approved by the Advisory Committee on Social and Economic Research in Agriculture.

Read by MORDECAI EZEKIEL at the joint meeting of the Econometric Society and Sections A and K of the A.A.A.S., Syracuse, New York, June 22, 1932.

THE background and purpose of this report are indicated by parts of the letter which Professor E. B. Wilson, President of the Social Science Research Council, sent to Mr. Tolley in June, 1930 informing him of the appointment of the committee and its duties:

> You are, of course, aware of the request by the Advisory Committee on Social and Economic Research in Agriculture for the appointment of a committee of this Council on mathematics to determine what mathematics should be taught to students of the social sciences. . . . The Committee on Problems and Policy in thinking the matter over decided that the time was not ripe for the appointment of a national committee but decided to appoint a sub-committee of the Advisory Committee on Agriculture which . . . should make a preliminary report on the subject. The only way I see of making progress is for the social scientists to know what they want, to explain to the mathematicians the types of problem they have to solve and the kind of mathematics they do use, so that the mathematicians on the committee may orient their thinking on a basis of reality.

Although the appointment of this Committee was the outgrowth of discussions in the Advisory Committee on Economic and Social Research in Agriculture concerning graduate training for students in Agricultural Economics and Rural Sociology, the Committee is of the opinion that the needs of students in Agricultural Economics and Rural Sociology for training in collegiate mathematics are not essentially different from the needs of students who specialize in other branches of economics and sociology, and that all students of the social sciences would find some work in collegiate mathematics quite helpful. Hence the needs of all students of the social sciences, so far as they are known to the committee, have been considered. Training in mathematics will be most helpful if obtained prior to courses in statistics and other subjects, in which mathematical terms, concepts, and methods, are, or may be, used. Hence the committee feels that students should plan, wherever possible, to obtain most if not all of their college training in mathematics in the early years of undergraduate work.

Since the committee was requested "to determine what mathematics

should be taught to students of the social sciences," they considered the desirability of recommending certain courses to be required of all students of the social sciences, or different courses to be required of different classes of students. However, after careful consideration, the committee reached the conclusion that such recommendations could be made only in very restricted form, because for some social science students it can not be demonstrated that substantial direct benefit would be realized from any college mathematics whatever. On the other hand, some students might profitably take more mathematics than is involved in an ordinary undergraduate mathematics major. There is need in nearly every institution doing graduate teaching or research in the social sciences, for at least one staff member actively engaged in social science work whose mathematical and statistical training is the equivalent of what might be required for a Doctor of Philosophy degree in mathematics and statistics.

In framing any general requirement for mathematics courses, it must be remembered that in the early stages of the undergraduate career—when the training in mathematics should be obtained—it is rarely possible for the student to judge whether his subsequent study and work will call for much or little mathematics. The committee believes, however, that a knowledge of the following phases of collegiate mathematics would be quite helpful and useful to a large proportion of the students in economics, and that many students in other social sciences would find it worth while to obtain the knowledge and training that can be acquired through the study of these phases of mathematics, providing it can be done without taking too much time from the study of other subjects.

1. *Logarithms:* for understanding special plotting methods and forms of equations; for numerical computation in connection with curve fitting and elsewhere.

2. *Graphs:* (as a mathematical tool) representation of tabulated data on ordinary and logarithmic papers; measurement of slopes and areas (especially in connection with frequency and cumulative curves); graphical determination of maxima and minima; representation of such three-variable relationships as are encountered in statistical studies, by means of three-dimensional diagrams and contours.

3. *Interpolation:* by reading graphs, (ordinary, semi-logarithmic, etc.); by proportional parts; and possibly by successive differences.

4. *Equations and forms of important curves:* straight lines; parabolic and hyperbolic curves; exponential and logarithmic curves; logistic curves; sine and cosine curves. The object should be to make the student sufficiently familiar with the forms and characteristics of the simpler curves to enable him to recognize readily the type of curve

suitable for fitting to any given body of data, and to appreciate the principal implications involved in the use of such curves.

5. *Probability:* combinations, binomial theorem, elementary probability; the normal probability curve—its form, table, and equation; probability and frequency distributions; probability and time series.

6. *Elements of differential and integral calculus:* the significance of a differential as a limit, as a rate or slope, as a frequency, etc.; partial differentiation; formula for differentiating elementary mathematical functions; procedure for determining maximum and minimum values; integration as the reverse of differentiation; relation between integration and summation; multiple integration.

7. *Curve fitting* (mathematical principles): plotting tabulated values, their logarithms, reciprocals, etc., to ascertain whether the tabulated points lie on a curve of simple form; determination of the curve through such points, or selected points within the scatter-diagram or chart; the method of least squares or other methods for obtaining the constants of curves of best fit.

The present organization and arrangement of the courses in many colleges and universities is such that fifteen to twenty semester hours of mathematics must be taken in order to obtain a knowledge of the above topics. Further, the courses are usually taught without any reference to the application of the mathematical concepts and terms to problems in the social sciences. In these circumstances comparatively few students in any of the social sciences feel justified in spending the time required to take these courses. In the judgment of the committee, the advantage of mathematical training to the majority of undergraduate majors in economics (the social science discipline in which mathematical training now appears to be most useful) is not sufficient to warrant the displacement of fifteen to twenty semester hours of other work, wisely chosen, by that amount of mathematics.

The committee is of the opinion that, if the mathematics courses be properly organized, the topics listed above can be taught to students who have had only ordinary high school mathematics in a form readily grasped by them and full of significance to them; also, that this can be done, with opportunity for ample practice, in a three-hour course running through two, or at most three, semesters. Illustrative material from the social sciences should be used freely, and *the concepts and processes should be presented in such a manner as to make clear their application in the social sciences.* The training obtained in such mathematics courses would help the student to acquire facility in reducing to definite terms the concepts and relationships encountered in the courses in the social sciences, and to do clear and logical thinking and reasoning.

This opinion is based on the experience of members of the committee in teaching mathematics, as such, and in teaching advanced courses in statistics to students who required to be taught a substantial amount of this mathematics along with the statistics, and on reports of experience with courses designed for this purpose in a number of colleges and universities.[4]

In view of all these considerations, the committee feels that if courses such as suggested above were available, most students in the social sciences would find it worth their while to take them. The committee has no hesitancy in recommending that this amount of mathematics should, if available, be fairly generally required of students specializing in economics and in some other of the social sciences. However, since only economists, political scientists, and mathematicians, were represented on the committee, we feel that representatives of other social sciences should be asked to give further consideration to this point.

Such training would prove of immediate benefit to students in subsequent courses in statistics, in finance, and in economic theory, if the subsequent courses presupposed a knowledge of mathematics and took advantage of that preliminary training. A substantial percentage of social science students would find further direct benefit from such a course amply repaying them for the time spent. Even those students who failed to realize direct benefit from the mathematical training in subsequent study and work would usually find the cultural and general educational value of such a course equal to that which might have been derived from an equivalent amount of time devoted to alternative subjects not directly related to their principal line of interest.

It should be said that the success of any such courses in meeting the need we envisage must depend upon the knowledge and ability of the instructor as much as upon the choice of topics for instruction. The instructor must possess the understanding of mathematics necessary to carry the students beyond the mechanics of formal mathematical processes to an understanding of the concepts involved, and must also possess the knowledge of social sciences to enable him not only to provide illustrations of uses of the mathematical methods in the social sciences, as well as in other sciences, but also to relate the mathematical concepts to concepts employed in the social sciences.

Preparation for courses in statistics.—Mathematics courses covering the above topics would be very helpful as a preliminary to a first course in statistics. The second encounter with the mathematical terms and concepts when taking courses in statistics will serve to give a more comprehensive grasp than would be possible with a single encounter. Moreover, with such mathematical preparation, the student in the

statistics course would be released from spending time in learning the mathematical topics *de novo*, and from distractions of attention incident upon such digressions. The statistics course could then utilize the usual amount of time to carry the student much farther in the knowledge of statistical methods, their possibilities and limitations; or the customary ground could be covered in a much shorter time.

Preparation for the study of economic theory.—Training in mathematics is useful to those who take undergraduate courses in economic theory in that it facilitates rapid and sound reasoning on many problems encountered in these courses. Many phases of economic theory, which seem quite involved and difficult to one who has not been trained to think in terms of equations, graphs, and small differences, can be stated quite simply in mathematical terms, and are easily understood by one who is able to grasp the meaning and significance of those terms.

For the sake of these advantages few instructors in economic theory have felt justified in urging any of their students to take the courses in mathematics, as customarily offered (trigonometry, college algebra, analytical geometry, and calculus).

The availability of briefer courses including the mathematics of principal use in the study of economic theory, and omitting unnecessary technique and complications, would go far toward swinging the balance of judgment to the other side.

Although some of the topics that would be covered have little direct application in the study of those phases of economic theory usually covered in undergraduate courses, most students who specialize in economics take a course, or courses, in statistics as well as in economic theory, and the committee is of the opinion that such students would be amply repaid for the time spent in taking mathematics courses such as those outlined above.

Preparation for research and teaching in the social sciences.—After obtaining the bachelor's degree, a large majority of students who specialize in the social sciences now enter upon business careers, or into other lines of work in which they find little direct use for the knowledge that can be gained in courses in collegiate mathematics. The inclusion of mathematics in the course of study of such students must be justified on the basis of the cultural training and mental discipline obtained, and on the basis of the usefulness of mathematical concepts and methods in other courses, such as economic theory, statistics, and accounting. With the student who expects to become a professional social scientist, however, the situation is quite different. The student who expects to become an instructor or researcher will usually spend two or more years in graduate work to obtain additional training in his chosen field. In this time, he will endeavor to prepare himself to

teach his subject to both undergraduate and graduate students, to carry on independent research, to make contributions to knowledge in his field, and to read and understand the contributions of his colleagues. Many such students will find a mathematical training equivalent to that suggested in the report absolutely essential, and some will find a considerably broader mathematical training very useful.

Students preparing to become instructors in economic theory, or to do research in a field requiring a comprehensive knowledge of economic theory, will find much more direct use for a knowledge of mathematics than will most of those whose collegiate training is limited to undergraduate study. In particular, it will make available the writings of agricultural economists and enable one to understand and evaluate the work of statistical analysts. Ordinarily, such students would do well to consider that it is essential to have mathematical equipment at least equal to that which would be obtained in the brief courses suggested for undergraduates, and plan to obtain this equipment as part of their formal training, even if it must be done in connection with their graduate study.

The student who expects to become a statistical analyst should have a thorough grasp of statistical theory and its mathematical background. In the analysis of social statistics, acquaintance with the fundamentals of statistical theory is essential in order to devise and apply sound methods, and to avoid faulty and fallacious analysis and interpretation of results. This implies the ability to read and understand the literature of statistical theory and method, and to modify established methods or develop new ones in problems to which existing methods are not exactly applicable. A thorough acquaintance with differential and integral calculus and the theory of probability, and an ability to handle abstract problems in these fields, are necessary for work of this character. For a student who is looking forward to a career as a statistical analyst, training in mathematics considerably broader than can be obtained in the abbreviated courses outlined above will be very helpful.

Similarly, one who expects to follow the writings of the mathematical economists, or to make contributions to theory in those branches of economics in which mathematical concepts and methods are used, must have a broad training in mathematics.

On the other hand, among those who are now doing research and teaching in the social sciences in fields other than statistical analysis and economic theory, there are undoubtedly many who have found little or no direct use for training in collegiate mathematics. Such is probably the case for most historians, many sociologists and political scientists, and some psychologists and economists. It is possible that

as further developments occur in the theory and research method of these disciplines, greater need for the general use of mathematical concepts and methods will become apparent, as has been the case in many of the natural sciences and, more recently, in statistical analysis and economic theory. A social scientist should not be swept off his feet by the appearance of a mathematical symbol or equation on the page of an article or in a textbook, no matter in which of the disciplines he is primarily interested.

In view of all these considerations, the committee feels that a student who is planning to become a professional worker in any of the social science disciplines will be well repaid for the time and effort spent in taking six to nine semester hours of collegiate mathematics, provided the courses are so organized that the phases of mathematics mentioned in the first part of this report could be covered in that time. Some would find it worth while to take the more extended courses in all these phases, even though they should require fifteen to twenty hours, if special abbreviated courses are not available.

The committee is cognizant of the fact that at present most of the graduate students in the social sciences have had no courses whatever in collegiate mathematics, but believes that many of these will find it worth while to make up their deficiencies even after beginning their graduate work. If abbreviated courses as outlined herein are available, those may be taken, along with more advanced courses which the student finds would be valuable to him. Some graduate students would, no doubt, be justified in taking enough of the more extended courses ordinarily offered in mathematics to acquire an understanding of the differential and integral calculus and the mathematical theory of probability.

Mathematics "requirements" in social science curricula.—Whether in any one of the social sciences, all students, or only certain more or less restricted groups, should be *required* to take courses in collegiate mathematics such as are suggested in this report, must depend to a considerable degree upon the degree of confidence with which need or lack of need for mathematical training can be predicted, upon the subjects which would be displaced if mathematics were required, and upon other circumstances which will differ as between disciplines within the social sciences and as between institutions.

The committee believes that at institutions where mathematics courses are available in which students can obtain a knowledge of the fundamental concepts of differential and integral calculus and the theory of probability, with the necessary preliminary training, in a total of six to nine semester hours, these courses should be made prerequisite to any course in statistics and to upper division courses in

economic theory, and the courses in statistics and economic theory should be organized so as to take advantage of the knowledge gained in the mathematics courses. Perhaps certain other courses in economics, and in the other disciplines as well, would, on further consideration, be included in the group for which the mathematics courses should be prerequisite.

In closing, the Committee suggests (1) that further study of the problem, with particular reference to the specific needs of the different social sciences and the coordination between courses in mathematics and the different social science disciplines, is highly desirable, and (2) that interested institutions be encouraged to proceed with the development of courses in mathematics designed primarily to meet the needs of students in the social sciences.

The following *resolution* was adopted unanimously:

"WHEREAS a committee of the S. S. R. C. has prepared a report, and whereas this report has been discussed in a joint meeting of social scientists and mathematicians under the auspices of Sections A and K of the A. A. A. S. and of the Econometric Society, be it

Resolved, that the members of the symposium urge the teachers of social science to lay more stress on mathematical training, and urge the teachers of mathematics to conduct instruction in mathematics in a manner really suited to the needs of the social scientists."

THE MEETING OF THE ECONOMETRIC SOCIETY IN ATLANTIC CITY, NEW JERSEY, DECEMBER, 1932

THE seventh meeting of the Econometric Society was held at Atlantic City, New Jersey, U.S.A., from Monday evening, December 26, 1932, through Wednesday afternoon, December 28. All sessions were held jointly with Section K of the American Association for the Advancement of Science.

The program for Monday evening consisted of four papers on subjects related to economic crises: "Harmonic Interaction of Economic Series," by H. T. Davis of the Cowles Commission for Research in Economics; "A Mathematical Theory of Industrial Fluctuations Caused by the Capitalistic Technique of Production and the Interdependence of Prices," by Edward Theiss of the Technical University of Hungary; "Community Expenditure and the Theory of Industrial Fluctuations," by Carl E. Thomas of the Federal Reserve Bank of New York, and "On the Feasibility of an Electrical Unit as an Optional Standard of Value," by Dr. J. P. Norton of West Haven, Connecticut.

There were two main features of interest in the paper by Professor H. T. Davis, i.e., a treatment of the interaction of economic series as though they were particles subject to elasticity forces, and the introduction of first and second order differences into the equations considered. In discussing this paper, Professor J. Shohat of the University of Pennsylvania stated that the first feature is novel and interesting, but that it is highly desirable to find a theoretical basis aside from analogy, for treating interacting economic series as dynamical systems. By a "theoretical basis" he meant one founded on the theory of probability and mathematical statistics. Professor Shohat pointed out further that there is difference of opinion in regard to the use of differences. G. Darmois, in his book *Statistique mathématique*, is in favor of using differences higher than the first in order to eliminate secular trends, whereas W. M. Persons, *Handbook of Mathematical Statistics*, is opposed to the variate difference method because "the method is based upon assumptions which cannot be retained even in simplest problems."

Dr. Edward Theiss presented a mathematical theory of fluctuations due to any of the following factors: (1) the duration of the productive process; (2) the longevity of the productive equipment; and (3) the interdependence of prices. In case (1) he introduced the length of production into the dynamic supply and demand equations of Evans, each

containing the time derivative of price. In case (2) he assumed the supply to depend upon a variable amount of productive equipment beside the price. He assumed the number of new machines added to the equipment per unit time to be a linear function of the time derivative of price. In case (3) he considered several commodities whose supply and demand functions depend upon all the prices and their time derivatives. The equations resulting from the equality of supply and demand in cases (1) and (2) lead to an infinite system of oscillations with constant or changing amplitudes depending upon the constants of the equations. For case (3) the system of oscillations is finite.

Mr. Carl E. Thomas presented the results of a statistical study on the relation between consumer expenditure and industrial fluctuations. Dr. J. P. Norton said that, theoretically, money should possess the following properties: (1) it should be relatively stable; (2) part of it should bear a small amount of interest, for example, bills of denomination equal to or higher than twenty dollars; and (3) it should be elastic enough to permit hoarding. Dr. Norton suggested that the kilowatt-hour might be a useful substitute for gold.

Tuesday morning a joint session devoted to mathematical statistics was held jointly with the American Mathematical Society. Dr. W. A. Shewhart of the Bell Telephone Laboratories presented the principal paper at this session. In discussing probability as a basis for action, he emphasized the difference between a prediction based on assumptions considered with reference to probability theory and a prediction based on assumptions that are known to be satisfied. Dr. Max Sasuly of the Brookings Institution described a method of smoothing economic series by moving averages. Professor C. H. Forsyth, of Dartmouth College, and Professor P. R. Rider, of Washington University, also presented papers.

The Tuesday afternoon program was devoted to miscellaneous papers on economics and statistics. Mr. Carl Snyder of the Federal Reserve Bank of New York presented a paper on momentum and inertia in economics, in which he discussed trends in industrial developments and in occupations in the United States. He concluded that available material indicates that the per capita growth of production and trade in the United States, from about 1800 to 1929, appears to have been at a rate varying but little from an average of about 2.8 per cent per annum. He said that vast sums of capital are needed to bring inventions into general use and these savings must come from the 5 or 10 per cent of the population who are able to save. Professor John H. Cover's paper on "Liquidation and Rehabilitation of the Consumer and Small Business" was read by title. Professor Truman Kelley of Harvard University described the ignorance of elementary mathe-

matics that he found in students of statistics. Professor J. Shohat said that a statistical institute is needed for teaching and research both in the mathematical theory of statistics and in economic problems which call for mathematical enquiry. Dr. Louis I. Dublin and Dr. A. J. Lotka of the Metropolitan Life Insurance Company presented a history of longevity in the United States. They traced the rapid growth in expectancy of life in the United States and pointed out that tuberculosis takes a smaller toll than ten years ago whereas organic heart disease takes a greater.

At 5:00 P.M. on Tuesday a joint session was held with the Mathematical Association of America and Sections A (Mathematics) and K (Economics, Sociology, and Statistics) of the A.A.A.S. At this time Professor G. C. Evans of the Rice Institute gave his address as retiring vice-president of the A.A.A.S. for Section K. This address, which was on the theory of money, is expected to appear in *Science*.

Wednesday was devoted to a symposium on the Stabilization of Employment, held jointly with Sections K and M of the A.A.A.S. Dr. Irving Fisher, president of the Econometric Society, delivered the opening address. He found that the correspondence between employment and price change is strikingly evident, especially whenever there is any great price disturbance, as during the war inflation and the two great deflations since the war. He declared that such a relationship is not empirical, because rising prices cause rising profits as a result of a business man's money receipts keeping pace with rising prices, while his expenses do not. The expenses are largely fixed—interest, rent, salaries, and even wages.

The next paper in this symposium was by Professor James W. Angell of Columbia University. In discussing monetary prerequisites for stable employment he showed, among other things, how the circular velocity of money is related to the net cash and bank deposits, the annual national money income, and the money volume of expenditures on consumers' goods and services when there is no voluntary new saving, and, also, when there is such saving. He concluded that it should be possible to regulate credit and currency so that employment will remain relatively stable.

Mr. E. J. Working of the United States Department of Agriculture presented a number of statistical studies of production and demand which showed contrasts between agriculture and other industries. In particular, he stated that agricultural production has been remarkably stable in the United States for the last ten years. In conclusion Mr. Working said: "Certainly the worst thing we can do is to try to restore price levels through further curtailment of production. This cannot succeed. If we are not to restore price levels, then I say that the most

important economic effort still before the world is to restore the level of production through adjustments to the lower price level, and to hasten that adjustment."

Professor Alvin H. Hansen of the University of Minnesota discussed international capital movements. He pointed out steps taken to prevent the collapse of prices on the return of various nations to the gold standard following the war. He said that the use of the gold specie standard by England resulted in an important economy of gold that helped to facilitate a return to gold at 40 to 45 per cent above pre-war prices. The gold exchange standard was adopted by most countries with England as the leading exception. The last three years have, however, witnessed the widespread collapse of the gold exchange standard, and there is no likelihood that it can be revived. Beginning with November, 1928, there was a persistent and nearly uniform decline of the gold stocks in the Central Banks and Treasuries of all countries combined, excluding the United States, France, Belgium, Holland, and Switzerland.

Professor H. L. Rietz of the University of Iowa considered actuarial aspects of unemployment insurance, and concluded that such insurance is unsound as long as economic conflagrations like the present one occur almost periodically. Professor Leo Wolman, of Columbia University, and Professor W. N. Loucks, of the University of Pennsylvania, discussed public works in their relation to employment stabilization. Six eminent engineers—Gerard Swope, C. F. Kettering, John Lyle Harrington, Walter Rautenstrauch, Dugald C. Jackson, and K. T. Compton—discussed such topics as employment assurance and insurance, the present-day viewpoint of science, self-liquidating public works, the effects of technological developments on prices, machinery and unemployment, and legislation for the reduction and relief of unemployment. The concluding paper was delivered by Dr. Royal Meeker, president of the Index Number Institute, who discussed and criticized various plans that have been offered for stabilizing employment. Papers included on the program of the symposium on the Stabilization of Employment have been published in a book by that name for the American Association for the Advancement of Science. Orders may be sent to the office of the Association in the Smithsonian Institution Building, Washington, D. C.

CHARLES F. ROOS

THE MEETING OF THE ECONOMETRIC SOCIETY IN CINCINNATI, OHIO, DECEMBER, 1932

THE Econometric Society met jointly with the American Statistical Association in Cincinnati, Ohio, December 29–31, 1932. The program was featured by Professor Irving Fisher's presidential address, a paper entitled "Interrelations of Demand" by Professor Henry Schultz of the University of Chicago, and a symposium on statistical forecasting at which four papers were presented. Attendance at the sessions reached a new high mark for Econometric Society gatherings. In his presidential address, entitled "Statistics in the Service of Economics," Professor Fisher said in part:

"A young economist in a western university, who sees eye to eye with me as to methodology, shocked and pained me about twenty years ago when he told me that his own enthusiasm had been so damped that he had practically given up his original life ambition of getting recognition for his contributions toward making economics into a true and useful science. But he was too easily discouraged. For, even at that time, those of us who had still earlier entered economics from the approach of the natural sciences, as contrasted with the approach from philosophy or history, had already noticed a great change. And today, we suddenly wake up to realize that the methods which have made physics a science have at last taken a vigorous hold on the rising generation of economists. To illustrate, I need only mention among many others Frisch, Divisia, Rueff, Schumpeter, Keynes, Bowley, Amoroso, Gini, Haberler, Leontief, Zawadzki, Kondratieff, Hotelling, Moore, Schultz, Roos, Crum, Ezekiel, and Rogers.

"Moreover, the establishment of the international Econometric Society two years ago, the meetings it has since held in Europe and America, and its journal, ECONOMETRICA, to appear next month, signalize this new birth of the scientific spirit in economics.

"As I understand it, this new outburst of the scientific spirit is not a new school but simply an adaptation to economics of methods already nobly tested in the natural sciences. I hope it may mark the beginning of the end of "schools" in the sense of partisan groups or cults. Both the Austrian deductive school and the German historical school have now ceased to command much enthusiasm; for some time the effort has been to find a satisfactory way to join together theoretical and historical studies. This effort has found expression through mathematics, and statistics, and mathematical statistics, the mathematics to express theory and the statistics to express historical facts.

"The Econometric Society, as stated in its constitution, stands for

'the advancement of economic theory in its relation to statistics and mathematics.' No one today needs to apologize for, or even defend, either statistics or mathematics as legitimate and helpful instruments in the service of economics. Both are found abundantly in any modern economic journal.

"It is sometimes said that the ability to predict is the final test of science. Unfortunately, as yet, neither economic laws nor economic data are sufficiently known to enable economists to make safe predictions. In both respects it is still a backward science as compared with many others. What we need in economics, or that branch of economics which we now call economic theory, is more of the old, old method which made astronomy, physics, chemistry, and recently biology, into true sciences. That goal, as I interpret the rising generation of students of economic theory, is where they are tending. Moreover they know where they are going and they are on their way."

The main body of Professor Schultz's paper "Interrelations of Demand"[1] was devoted to a detailed statistical analysis of the interrelations of the demands for the four feed crops, barley, corn, hay, and oats, for the period 1896–1914. The analysis was undertaken with the following objectives in view: (1) to compare the theoretical properties of demand functions for competing and completing goods, (2) to analyze the concrete statistical demands for the foregoing commodities with the view of determining which of them are completing and which are competing, (3) to see whether and to what extent the theoretical relations are reflected in the observations, (4) to call attention to certain new problems in statistical methodology, and (5) to indicate the practical significance of studies in the interrelations of demand, especially their bearing on the problem of taxation.

The theory underlying Professor Schultz's statistical work may be summarized as follows:

Let the total utility function of an individual be

$$\phi = \phi(x_1, \cdots, x_n), \tag{1}$$

where $x_1, x_2, \cdots,$ are quantities of commodities $(X_1), (X_2), \cdots$. Then any two commodities (X_i) and (X_j) may be classified as completing, independent, or competing, according as

$$\phi_{x_i x_j} \equiv \frac{\partial^2 \phi}{\partial x_i x_j} \gtreqless 0. \tag{2}$$

To translate (2) into conditions on the demand functions, we make use of the well-known relation

[1] To be published in the *Journal of Political Economy*.

$$\phi_{x_i} = \frac{\partial \phi}{\partial x_i} = m y_i, \tag{3}$$

where y_i is the price of x_i and m is the final degree of utility of money, which is assumed to be constant.

By virtue of (2), two commodities are defined as completing, independent, or competing, according as

$$\phi_{x_i x_j} = m \frac{\partial y_i}{\partial x_j} \gtreqless 0, \tag{4}$$

where

$$y_i = y_i(x_1, \cdots, x_n) \tag{5}$$

is the demand function for the i-th commodity.

Since the function ϕ is assumed to exist, it follows that $\phi_{x_i x_j} = \phi_{x_j x_i}$. This leads, by (3), to the condition

$$\frac{\partial y_i}{\partial x_j} = \frac{\partial y_j}{\partial x_i}. \tag{6}$$

Professor Schultz stated that the integrability condition (6), which was first obtained by Professor Hotelling by a somewhat different method, is of fundamental importance in the study of the interrelations of demand.

Thus, for example, if the demand functions are linear:

$$y_1 = a + bx_1 + cx_2$$
$$y_2 = a' + b'x_1 + c'x_2,$$

the integrability condition (6) tells us that for both competing and completing commodities, the parameters c and b' must be equal to each other. They are both positive when the commodities are completing, and both negative when the commodities are competing. The parameters b and c' are, of course, negative.

In conclusion he pointed out that his calculations show that for hay and oats the two terms of the integrability condition (6) differ significantly not only in numerical value but also in sign, thus giving rise to several difficult questions.

Professor M. D. Anderson of the University of Florida, who was unable to be present, transmitted to the meeting some comments on Professor Schultz's paper. He questioned the assumption that the marginal utility of money is sensibly constant, and pointed out that there might be cyclical changes in the utility of money which would upset the measurement of related demand in somewhat the same way that a shifting demand confuses the measurement of unrelated demand for a single commodity.

In the discussion of the paper of Professor Schultz it was pointed out by Professor Harold T. Davis of Indiana University that no proof ex-

ists that the utility function is a point function rather than a *fonctionelle*, that is to say, that it depends upon the values of the commodities rather than upon the path by means of which the values were attained. The search for a measure of utility in econometrics was compared with the search for the functions in thermodynamics. Thus, in the differential relationship $dE=dQ+dW$, where E is the internal energy, Q is heat, and W is work, the important question was: which is the *fonctionelle* and which the function? The discovery that dQ/T was an exact differential, and hence that entropy was a function, was a supreme achievement. It was equally important to know that W was a *fonctionelle*. The suggestion was made by Professor Davis that the character of the utility concept could probably be explored with profit by means of line integrations in the commodity space, employing the technique of Vito Volterra's functions of lines.

Commenting on Professor Schultz's paper, Professor Wirth F. Ferger of the University of North Carolina said:

"I congratulate Dr. Schultz, as he extends the field of the mathematical-statistical theory of demand study to the inclusion of intercommodity price-quantity relations, on insisting on a continual checking of the mathematical theory by reference to actual data. Not only does he avoid the arbitrary assumptions implicit in mathematical equations involving indefinite functions or functions selected for their operational convenience, but a valuable by-product of the dual method is here shown; the conflicting and contradictory results found in the relations of oats and hay suggest the necessity for additional study of both theory and data. I raise detailed points concerning only two aspects of Dr. Schultz's paper; a clarification of our terminology so that a 'change in demand' will have only one meaning, and that its most significant one, is desirable; and the differentiation of data employed (inclusion or exclusion of 'end stocks') between studies of 'flexibility of price' and 'elasticity of demand,' respectively, is suggested. Dr. Schultz combines in this paper to a remarkable degree mathematical astuteness and economic reality."

The symposium on Statistical Forecasting began with a paper, entitled "Can Stock Market Forecasters Forecast?,"[2] by Alfred Cowles 3rd of the Cowles Commission for Research in Economics. He stated that 16 financial services, in making some 7500 recommendations of individual common stocks for investment during the $4\frac{1}{2}$ years ending June 1932, compiled an average record that was worse than that of the average common stock by 1.43 per cent annually, and that statistical tests of the best individual records indicated that they were probably

[2] To be published in Econometrica

the results of chance. Twenty fire insurance companies, in making a similar selection of securities, achieved an average record 1.20 per cent annually worse than that of the general run of stocks, and their best individual records were not very much more impressive than the records of the most successful of the 16 financial services.

Mr. Cowles reported that William Peter Hamilton, formerly editor of the *Wall Street Journal*, publishing forecasts of the stock market based on the Dow Theory over a period of 26 years from 1904 to 1929 inclusive, achieved a result better than what would ordinarily be regarded as a normal investment return, but poorer than the result of a continuous outright investment in representative common stocks for this period. On 90 occasions Hamilton announced changes in the outlook for the stock market, and half of these predictions were unsuccessful. Mr. Cowles further reported that 24 financial publications engaged in forecasting the stock market during the $4\frac{1}{2}$ years from January 1928 to July 1932, failed as a group by 4 per cent per annum to achieve a result as good as the average of all purely random performances. He concluded that various statistical tests indicated that the most successful records among these 24 forecasters were little, if any, better than what might be expected to result from pure chance, and that there was some evidence to indicate that the least successful records were worse than what could reasonably be attributed to chance.

In discussing the paper of Mr. Cowles, Professor Harold T. Davis of Indiana University explained the technique by means of which it was found possible to measure the probable attainment of the forecasters.

"The first step in this," said Professor Davis, "was to compute the standard deviation, as a function of time, of a group of N stocks from the market average:

$$\sigma(t) = \sigma^{(0)} + \sigma^{(1)}t + \sigma^{(2)}t^2 + \sigma^{(3)}t^3 + \cdots, \quad \frac{d}{dt}\sigma(t) > 0.$$

"By studying the behavior of a list of 60 stocks chosen at random, half of them during the relatively quiet months of 1928, the others during the mercurial period from July 1, 1929 to July 1, 1930, the desired deviation was computed to be: $\sigma(t) = 5.42 + 1.58\,t$, t in units of four weeks.

"If $d(m)$ is the observed deviation of a group of N stocks from the market at the end of 4 m weeks, the sign being chosen positive, and if we write $I(s) = (1/\sqrt{2\pi}) \int_{-s}^{s} e^{-1/2t^2} dt$, then the probability of attaining by random chance the same or a greater deviation would be, $P(m) = 1 - I[d(m)\sqrt{N}/\sigma(m)]$. Since we are concerned mainly with positive deviations, the probability that the deviation will be both positive and of magnitude $d(m)$ is given by, $p(m) = \frac{1}{2}P(m)$.

"In order to apply this theory to the complete record of a service, its M periods of forecasting are divided into two groups, in A of which the forecast has equalled or exceeded the market, and in B of which it has been below the market. Then the chances to do better or worse than the service are computed as explained above. Averages of each set of probabilities are then made, these averages being justified by considerations which underlie the theory of Lexis and Poisson distributions. Let us call the average for the positive record S, and that for the negative record T.

"Now the probability that a service will be right A times and wrong B times out of $A+B=M$ times, is equal to the first $B+1$ terms of the binomial $(\frac{1}{2}+\frac{1}{2})^M$. Let us call this probability K.

"Finally the probability must be computed which represents the chance that a random service will be on the positive side of the market A times and will achieve a record as good as the service under consideration. This probability is obviously of the form: $P=K(1-Q)$ where Q (the chance to do worse) is to be computed. A reasonable measure of Q, based on considerations of inverse probability, may be chosen to be: $Q=(A/M)S+(B/M)T$."

Victor S. von Szeliski of the Lehman Corporation presented a paper entitled "The Statistical Analysis of Stock Prices."

"The technical action of the stock market," said Dr. von Szeliski, "has so far been studied almost wholly by graphic art which should be supplanted by strict statistical methods. To be acceptable, whatever methods are evolved must be (1) objective, (2) numerical, and (3) permit of indefinite repetition of essentially similar observations.

"Point changes in the price p of stocks at different price levels are not comparable. A change of 1 point in Anaconda has not the same technical significance as an equal point change in International Business Machines. Let q be a function of p such that numerically equal Δq's have similar technical significance for all values of q and p. Clearly, $\Delta q \neq \Delta p = \Delta p/p^0$. The first suggestion was that $\Delta q = \text{const.} \times \Delta \log p = \text{const.} \times \Delta p/p'$. More recently, the square root law has been proposed: $\Delta q = \text{const.} \times \Delta p/p^{1/2}$. Observation of the high-low range of stock prices as a function of price on the first day or two of important moves (when influences are mainly technical) favors the logarithmic law as against the other two. But the best law appears to be a three-quarters power law: technically equivalent price changes are proportional to the $\frac{3}{4}$ power of the price level at which they occur.

$$\Delta q = \text{const.} \times \Delta p/p^{3/4}$$

Integrating:

$$q = C_1 p^{\frac{1}{4}} + C_2.$$

"The volume in a quiet stock like Air Reduction is evidently not comparable with that of an active industrial like United Aircraft, still less with the total shares traded. The obvious first step is to express the volume for a single day, v, as an index, with the average volume for some past period = 100. In this study, the base volume, V, is the average daily volume during the immediately preceding nine days. $\lambda = \log v/V$ is used, rather than v/V, as it has a more symmetrical frequency distribution, and is unlimited in both directions. A study of the dispersion of λ as dependent on V shows decreasing dispersion with increasing V. The A. D. of λ for $V = 10{,}000$ is approximately .2; for $V = 1{,}000{,}000$, about .1. (A. D. calculated from O as origin.) Observations on λ for American Can, U. S. Steel, General Electric, Consolidated Gas, General Motors, and the market as a whole, suggest the relationship:

$$A.\ D._{\lambda} = 0.9 \cdot \cdot \cdot - 0.15192 \log V.$$

If we express each λ in A. D. units, we get quite comparable volume measures for inactive stocks, active trading favorites, and the market as a whole. This measure is the volume index, vi

$$vi = \frac{\log v/V}{AD_{\lambda}(V)} = \frac{\lambda}{\lambda'}.$$

It is based on the assumption that equiprobable volumes are equisignificant, technically.

"What are the statistical characteristics which distinguish the first day or so of a temporary rally from the first day or so of a considerable advance in the market? Measure, for each such rally, (1) its volume in terms of the volume index, (2) its extent in per cent using closing prices of the Standard Statistics Index of Ninety Stocks. (Since the index is not an average price, the three-quarter power price change law cannot be used.) Plot these coordinates as points on a graph, using the X-axis for volumes and the Y-axis for rally. Color the points associated with false rallies red, the points associated with larger rallies blue, and it will be noted that the blues congregate to the right of the chart and down, the reds to the left and up. The probability that a given rally will be followed by still higher prices is the ratio, for that region of the chart, of the number of blue points to total points. The probability is thus a function of vi and $\Delta p/p$. For certain regions the probability is very large or very small; for an intermediate region, about $\frac{1}{2}$. The problem of statistics in stock market research is to decrease the width of this intermediate zone."

"The Use of Mathematics in Business Forecasting" was the subject of a paper given by Warren F. Hickernell, editor in charge of business

forecasts published by the Brookmire Economic Service, 1911–1916, and the Alexander Hamilton Institute, 1916–1928.

"The fact that it is impossible to establish periodicity of crises and depressions," said Mr. Hickernell, "limits the use of mathematics in the field of business forecasting. Man-made financial policies or political developments may destroy the value of forecasts based upon the study of sequence and lag as revealed in trade and financial statistics. Thus the only basis of statistical forecasting within our reach is the barometric cross-section method, certain cross-section pictures, or patterns, of statistical barometers being assumed to indicate favorable or unfavorable conditions regardless of previous lag or sequence.

"The ordinates of curves of trade and banking time series, with secular trend and seasonal variation removed, had definite value in business forecasting before the World War. In the United States the rigid reserve requirements of the National Bank Act controlled the trend of bank loans and deposits. In Europe all leading countries were on the gold standard, and the Bank of England controlled the London money market, usually, according to an established rule. The application of mathematical formulae to business cycle statistics was favored by the fact that credit policies were so universally adjusted to the impersonal authority of gold reserves.

"Mathematical barometers have declined in prestige since 1914 because changes in American banking laws have rendered obsolete the pre-war 'normal' in banking series and because gold reserves in the United States since 1919 have been heavily 'adulterated' by inclusion of abnormal amounts of foreign funds. In 1928–29 Europe virtually owned half the gold in the United States, and the statistician could not know whether this foreign gold would remain as a credit base or be withdrawn on short notice. Confident forecasting was impossible.

"Those statisticians who used published statistics of gold reserves in the United States without removing the 'foreign content,' which had steadily increased during the blowing of the Dawes Plan Bubble (1924–29), were entirely unprepared for the gold raid of 1931.

"At present writing, the United States owns practically all of the gold it possesses. But in continental Europe, gold statistics cannot be used as published. A large amount of gold from Austria, Germany, Great Britain, and Japan, has accumulated in Switzerland, Holland, Belgium, and France. The business forecaster must remove the 'foreign content' from continental gold reserves in estimating the trend of economic conditions.

"During the past decade, business forecasting has required diligent analysis of money market abnormalities. At present, allowance must be made for the fact that the velocity of circulation of money and

credit is far below average, and for the abnormal withdrawal of gold from effective economic use. When the 'foreign gold' has been removed from continental reserves to Berlin, London, Japan, the United States, and South America, and the velocity of circulation has recovered to 'normal,' the use of mathematical formulae will become increasingly valuable in preparing barometric cross-section patterns."

The closing paper of the symposium was presented by Lewis A. Maverick, Assistant Professor of Economics, University of California at Los Angeles. It was entitled, "Time Series: Their Analysis by Successive Smoothings."[3]

"It has been suggested by Ragnar Frisch," said Professor Maverick, "that a trend or smoothing line may be drawn to pass through the points of inflection in the original curve, and that, if the smoothing line be itself examined, it will in turn show fluctuations which will necessarily be marked by points of inflection. These points may be connected to give a trend or smoothing line of the second order, and it may be submitted to the same process. The first smoothing line is freed from the fluctuation of shortest period, but contains all fluctuations of longer period; the second smoothing line is in turn freed from the lowest order fluctuation remaining in the smoothing line of first order.

"In the present paper the concept is accepted of a succession of smoothing lines which set apart the fluctuations into homogeneous orders. The criterion of points of inflection is accepted as but one of several criteria, aiding in the location of the successive smoothing lines; the shortcoming lies in the indeterminateness of the location of the points of inflection. Added criteria employed are moving averages, minimum radius of curvature, and 'bounds,' which are two curves that touch, respectively, the successive peaks and the successive troughs.

"Each order fluctuation may be studied for period and amplitude and a standard pattern may be determined. In forecasting, the highest order trend may be continued forward, and those of lower orders superimposed in the form of their standard patterns. In correlation, the study may be carried on separately for each order fluctuation. In constructing index numbers, the selection and weighting of the component series may be undertaken separately for each order fluctuation, according to the significance of the series for the fluctuation of the particular order."

ALFRED COWLES 3RD

[3] To be published in ECONOMETRICA.

THE ECONOMETRICA COMMITTEE ON SOURCE MATERIAL FOR QUANTITATIVE PRODUCTION STUDIES

THE coefficients of production, the curves of marginal and average cost, and the functions of marginal productivity, have played an important part in economic theory, but we have, as yet, little knowledge of the numerical values of these coefficients and functions, and of their applicability to the current practice of business. To examine this situation, an international committee was brought together during the Paris meeting of the Econometric Society, at the initiative of the editor of ECONOMETRICA, and charged with an experimental survey of the availability of statistical material in this field, and the amenability of such material to the purposes of the economist. Three kinds of material are envisaged: (a) data already published in a generally accessible form; (b) data gathered together by research institutes, schools of business administration, government departments, and the like, but not made public; (c) data accessible to direct inquiry into the accounts and costing practices of business undertakings.

The committee would be happy if through its inquiry it could establish the existence of a body of material sufficiently objective to be indexed and possibly published for the use of econometricians in determining numerically the curves of marginal and average cost and the like. But it does not proceed on the assumption that any such material exists, and it is especially concerned with the problem of how far the analysis must depend upon an intimate acquaintance with the history and practices of the productive unit to which they refer. The committee has, therefore, to examine such factors as changes in technique, agreements with trade unions on working practices, and the framework of cost accounting, in their bearing on the significance of the data. It will consider also such indirect evidence as is yielded by the prices at which, at different levels of output, the productive unit has been willing to undertake an increment of output.

In accordance with the exploratory character of the committee's inquiry, no attempt has been made to map out the whole field: only a sample of countries in Europe has been taken. At the European meeting of the Econometric Society in 1933 a report will be presented in which the results of the inquiry will be surveyed, and the scope of further action will be determined in the light of these findings.

The present membership of the committee is as follows:

Denmark: Prof. F. Zeuthen, University of Copenhagen.

France: M. Georges Lutfalla, Actuaire-Contrôleur Adjoint au Ministère du Travail.

Germany: Dr. Erich Schneider, University of Bonn.

Great Britain: E. H. Phelps Brown, New College, Oxford (*Chairman*).

Holland: Dr. J. Tinbergen, University of Leyden.

Poland: Dr. Oskar Lange, University of Cracow.

Switzerland: Dr. Hans Staehle, International Labour Office.

Members at large: Prof. Ragnar Frisch, University of Norway, Dr. Jakob Marschak, University of Heidelberg.

The committee will very gratefully welcome the communication of all suggestions and information.

FRANCESCO FUOCO, OPPONENT OF J. B. SAY ON THE USE OF ALGEBRA IN POLITICAL ECONOMY

By Gustavo Del Vecchio

Paper read at the meeting of the Econometric Society, Lausanne, September, 1931

I would like to call the attention of the meeting to the Italian writer Francesco Fuoco, who, in the fourth of his *Economic Essays* (Pisa, 1827), discussed at length "The Use of Algebra in Political Economy," (I, 61–120). The table of contents of his book at once gives some idea, perhaps an exaggerated one, about its contents:

Preface.

Chapter I.—Use of Algebra.

Chapter II.—Is Algebra adapted to apply to the objects of Political Economy?

Chapter III.—Examples of some of the principal applications of Algebra to the aims of Political Economy.

Chapter IV.—How far one can usefully extend the application of Algebra to Political Economy?

Chapter V.—Consequences, observations, and summing up, of the essay.

Fuoco is mentioned by Cossa, Jevons, and in Palgrave's *Dictionary of Political Economy*, but if he really treated all these subjects, eleven years before Cournot, in a conscious and constructive way, he could rightly be called an unknown economist.

Truly, his actual treatment of the problem is rather careless, and it exhibits several mistakes which mathematical economists ought to avoid. But still his exact general argument cannot be overlooked, and we should not forget his service in opposing the view adopted too hurriedly (on the occasion of the unhappy attempt of Canard) by J. B. Say, who espoused the position that mathematics cannot find a place in economic research. The confutation of such a great authority as Say in a question of this importance is sufficient to ensure a place for the Italian writer in the history of economic thought.

Bologna, R. Università

ALFRED MARSHALL, THE MATHEMATICIAN, AS SEEN BY HIMSELF

After the publication of the first volume of his "Cours" in 1907, Professor Colson became acquainted with the work of Alfred Marshall, and was much struck by the identity of their methods. Colson wrote to Marshall to explain why his work contained no reference to the latter. In his letter, he asked whether Marshall was a mathematician who had turned economist, or was, like Colson himself, an economist who had merely found it useful to introduce certain mathematical formulae into his analysis. Marshall answered in the highly interesting letter which follows. This letter shows the profound influence that mathematical training had on his whole economic work, as it had also, but in a different way, in the case of Colson himself. It was as a high official of the Conseil d'État and at the Ministère des Travaux Publics, that Colson was led to apply himself to Political Economy. He naturally built on the strong mathematical foundations he had laid at the École Polytechnique and the École Nationale des Ponts et Chaussées. Thus he reached a "scientific economics" through a route quite different from Marshall's, and the agreement of the two is very significant.

Balliol Croft, 6 Madingley Road, Cambridge

Dear Sir,

I write in English, because I start tomorrow for South Tyrol, and must finish some writing before I start: and I write slowly in French.

Briefly—I read Mill's *Political Economy* in 1866 or '7, while I was teaching advanced mathematics: and, as I thought much more easily in mathematics at that time than in English, I tried to translate him into mathematics before forming an opinion as to the validity of his work. I found much amiss in his analysis, and especially in two matters. He did not seem to have assimilated the notion of gradual growth by imperceptible increments; and he did not seem to have a sufficient responsibility—I know I am speaking to a mathematician—for keeping the number of his equations equal to the number of his variables, neither more nor less. Since then I have found similar matters not quite to my taste in the economic work of nearly all those who have had no definite scientific training.

At that time and for long after I knew very little of the realities of economic life. But I worked at what I regard as the central problem of distribution and exchange. Before 1871 when Jevons' very important *Theory of Political Economy* appeared, I had worked out the whole skeleton of my present system in mathematics though not in English. My mathematical Note xxi concentrated my notions: but the greater part of the earlier notes and especially Notes xiv–xx were evolved in substance about the same time. These contained the substance of my doctrine of Substitution; though I did not make use of that term till long after.

As I have said in my original Preface, I owed much to the mental discipline afforded by Cournot; but the one book which really guided me was written by a landowner, who had very slight knowledge of mathematics, and indeed occasionally talked great nonsense in them. It was von Thünen, as you already know.

In 1871 I married; found myself committed to writing a cheap popular book, which was necessarily superficial, and which I loathed. After a few years I became very ill, and expected not to be able to write anything considerable. So I took out the diagrammatic appendices which I had written for my book on International Trade; and decided to edit them, showing their uses, and above all their *limitations:* after that was done I expected to depart this life. But I slowly recovered. And so the purely analytical work in Book v of my Principles, with a part of Book III, were the kernel from which my volume expanded backwards and forwards to its present shape. The Austrian School is on lines somewhat similar to a part of mine. But I knew nothing of Carl Menger till my own ideas were nearly in full shape: and Böhm Bawerk and Wieser were at that time at school, or students at the University.

Unfortunately, the French translation of my *Principles* Vol. I was partly published before I received an unexpected call for a new edition which appeared in 1907.

The Preface to that indicates the nature of the changes made in it, and in my plans generally. Age is beginning to tell on me; and I resigned my Professorship a year ago, in order that I might make more progress with *National Industries and International Trade*, which I am writing slowly.

The Austrians, and especially Böhm Bawerk, annoy me (though there is much in their work which I admire) by pretending to have revolutionized the bases of economics; whereas nearly all their doctrines appear to me to have been latent in the writings of the "classical" economists. Also they offend against my mathematical instincts: though I have now ceased to use the mathematical language easily.

Yours very truly

ALFRED MARSHALL

I know well your *Transports;* and have often recommended it. But I do not know, alas!, your *Cours;* and now I have no more any pupils. I send a Memorandum that may interest you.

INSTRUCTIONS TO CONTRIBUTORS REGARDING PAPERS FOR ECONOMETRICA

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ANNUAL SURVEY OF STATISTICAL TECHNIQUE.

DEVELOPMENTS IN SAMPLING THEORY

By W. A. Shewhart

INTRODUCTION

It was with considerable hesitation that I accepted the invitation to write this introductory survey on statistical technique, realizing the extensive annual additions on the formal or mathematical side and the serious problems of induction involved in appraising the usefulness of these formal developments. My experience as a member of a recent committee of the American Statistical Association on Research in Statistics had done much to make me sensitive not only to these difficulties, but also to those arising from the divergence of opinion among statisticians as to what constitutes statistical technique. In fact, two members of this committee attached to the committee's report[1] statements emphasizing the fact that statistical technique in its broader sense should include not only that of mathematical analysis, but also techniques of questionnaire drafting, sampling under field conditions, tabulation of data, computation, and planning.

One of the first things that I undertook was to survey the rather extensive literature that has grown up on the application of statistics in economics and some of the associated sciences, for the purpose, among other things, of getting a better picture of what economists include under the subject of statistical technique. Beginning at least as far back as 1925, the annual addresses[2] of the past presidents of the American Statistical Association, including those of Chaddock, Ayres, Day, Snyder, Wilson, Rorty, and Ogburn, were concerned to a large extent with the discussion of some phases of methodology. In 1930 the program of the annual meeting of this Society contained numerous papers on statistical methodology contributed by men in various fields.

After reading this series of addresses and papers, together with some similar ones appearing in other statistical journals, I have become more convinced than ever that *the* answer to the question, What is statistical technique? cannot be given. For example, Ogburn in his presidential address, 1931, states that statistical method must vary according to subject matter, and that statistics must be taught in various departments where it is applied. He also says that, for these reasons, statistics tends to break up into separate disciplines somewhat as philosophy did. In others of these articles, we find evidence of somewhat similar beliefs.

[1] Cf. *Proc. Amer. Stat. Assoc.*, March, 1932, pp. 252–259.

[2] Printed in the March issues of the *Jour. of the Amer. Stat. Assoc.*, 1926 to 1932, respectively.

Furthermore, such terms as common sense, logic, judgment, and scientific method, are used by some of these authors as though they were apart from, and of greater significance than, statistical method or technique. In fact, Rorty, 1930, states that a statistician should be instinctively and primarily a logician and a scientist in the broader sense, and only secondarily a user of specialized statistical techniques.

It seems desirable, therefore, that this first review of statistical technique should attempt to throw some light on the developments taking place in the conceptual relationship between logic and scientific method on the one hand, and statistical technique on the other, and should indicate in a general way the lines of development in attacking those problems which may perhaps be considered as belonging exclusively to the field of statistical methodology. This need for unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems is emphasized as one of the objectives of the Econometric Society.

THE FUNDAMENTAL PROBLEM OF INDUCTION

Both natural and social scientists are interested in the *discovery* of so-called facts, relationships, and causes. Since the existence of such entities must be induced from observed data, scientists are interested in gathering, presenting, and interpreting data, both quantitative and qualitative. This problem is common to the fields of scientific method, logic (including the theory of knowledge), and statistical method. To this extent the three fields are closely related.

It is, of course, common to distinguish between two kinds of induction: intuitive and *rational*. By mutual agreement, the discussion of technique in any one of these three fields is usually limited to that having to do with rational induction. It is also true that modern students of induction such as Keynes, Nicod, Ramsey, Broad, Johnson, Lewis, Whitehead, Russell, and Eddington, as well as scientists in general, are in agreement that rational induction can never lead to certainty. In other words, they are in agreement that no matter how many data we gather, analyze, and interpret, we can never do more than say that such and such is probably a fact, or such and such relationship probably exists, or such and such is probably the cause of some event.

The end of research in this sense is the establishment of a judgment that such and such is a fact, relationship, or cause, in which we can believe with a certain degree of rational belief or probability. We shall assume that if the inference or judgment P is connected to the evidence Q through some probability relation, there is an objective rational degree p_b' of belief in P upon the evidence Q. To the extent to which this assumption is justified, the ultimate object of research in any field is

the establishment of the rational degrees of belief to be associated with such judgments.

Furthermore, it is generally agreed that the objective degree of belief p_b', in an inference or judgment P, is not an intrinsic property like truth, but inheres in the inference or judgment through some relation to the evidence Q. In other words, we are not justified in saying that p_b' is the degree of rational belief or probability in the judgment P. We must add that this is the degree of rational belief upon the evidence Q.

Limiting ourselves to the quantitatively measurable characteristics of a phenomenon, it is customary to distinguish conceptually between the phenomenon which exhibits under presumably identical conditions the same magnitudes of physical characteristics, and the phenomenon which under presumably identical conditions exhibits stability of the measurable characteristics only in the statistical sense. For a considerable period of time phenomena in the field of physics were assumed to behave in accord with the first concept. Contrasted with the "exactness" of physical science there grew up the concept of the "inexactness" of most phenomena in the field of social science, and in such other fields, for example, as biology and psychology. For example, in the first edition of Yule's classic, *An Introduction to the Theory of Statistics*, 1910, he characterized inexact phenomena as those arising from a multiplicity of causes, and stated that statistical methods are those adapted to the elucidation of quantitative data affected to a marked degree by such a multiplicity of causes.

Perhaps it is traceable to such distinctions, made not only by Yule but by many other writers before and since, that many have come to consider that statistical methods have to do with only a limited class of data, and that they are not necessarily and inherently associated with scientific method as such. Today, of course, we appreciate as never before that even in the field of physics, it is not possible to duplicate phenomena exactly. Accordingly, in all fields of science we have been forced at one stage or another to introduce the statistical concept of constancy, involving the following assumption: If a sequence of events happens under the same essential conditions, where an event is characterized in terms of c quality characteristics $X_1, X_2, \cdots, X_i, \cdots, X_c$, then the ratio p of the number of events in the sequence of n such events having characteristics falling within the respective ranges $X_1 \pm \frac{1}{2}dX_1, X_2 \pm \frac{1}{2}dX_2, \cdots, X_i \pm \frac{1}{2}dX_i, \cdots, X_c \pm \frac{1}{2}dX_c$, to the total number n of such events, approaches a definite limit[3] p', as the

[3] Of course, this limit is inherently different from a mathematical one in that we never reach a value n_0 of n such that for $n \geqq n_0$, the difference $|p - p'|$ becomes and remains less than some previously assigned positive quantity ϵ.

number n increases indefinitely, where this limit is termed a statistical probability.

That things are not reproducible in the exact sense of the older natural sciences, complicates the problem of induction, and at the same time opens up a new field of investigation which seemingly should be considered as the field of statistical methodology, or technique. From this viewpoint we readily distinguish three characteristic statistical problems: specification of objective distributions, deduction of distributions of statistics of samples of size n drawn at random from a given specified universe, and statistical induction.

In other words, instead of assuming the objective constancy of any variable X observed under essentially the same conditions, we assume an objective probability distribution which may be either continuous or discontinuous. For the purposes of illustration, let us assume that the objective probability distribution is a continuous function f' of X and m' parameters, or, in other words, that the statistical probability dp that an observed value of X will lie within the range $X \pm \frac{1}{2}dX$ is

$$dp = f'(X, \lambda_1', \lambda_2', \cdots, \lambda_{m'}')dX. \tag{1}$$

The variable X may, of course, be an observed regression coefficient or estimate of a parameter in a physical law as well as a direct measurement. A fundamental problem is the discovery or inference of (1) from an observed set of data.

SPECIFICATION

The problem of specification is essentially that of fixing upon the functional form f' involving the m' parameters of unknown[4] magnitude to be used as a hypothetical basis for the solution of the other two problems—distribution and induction. In other words, it is the second step in the application of scientific method involving observation, hypothesis, deduction, and experimental verification. Obviously, the scientific investigator must in any field construct a mental model of phenomena he observes, and then test its consistency with itself, and its concordance with the results of further experiment. An hypothesis so conceived is not something fixed, but something subject to change if at any stage in an investigation the experimentalist becomes convinced that the observed data are not consistent therewith.

This concept of the rôle of hypothesis has thoroughly permeated the field of natural science—particularly the field of physics. However, only within the last few decades has it begun to influence the work of the statistician, as may be illustrated by reference to the treatment of

[4] Of course, as a basis for some statistical inferences we assume both the form f' and the parameters to be known.

frequency curves in texts on applied statistics. Such treatments often start by discussing methods of collecting and tabulating observed frequency distributions, and of summarizing these in terms of measures of central tendency, dispersion, and skewness, as though these processes were entirely separate from interpretation. Obviously, however, the interpretation of an observed distribution will depend upon whether or not there is justification for believing that it was produced at random from a constant system of chance causes. Furthermore, the statistics to be used in summarizing the observed data in respect to central tendency, dispersion, and similar characteristics, will depend upon the assumption adopted in respect to the constancy of the chance cause system and the functional form of the objective distribution function. The older treatment of frequency curves was more from the viewpoint of graduation of observed distributions than from that of establishing the necessary hypothetical basis for testing the assumptions of constancy of the underlying chance cause system and of giving efficient ways of summarizing the essential information which recent developments in statistical technique emphasize.

One of the earliest attempts at specification is that having to do with the establishment of the so-called law of error. Later the development of generalized frequency curves was attacked from several directions[5] by Gram (1879), Thiele (1889), Charlier (1905), Pearson (1895), Edgeworth (1896), Fechner (1897), and Bruns (1897). Out of these efforts belonging to the nineteenth century have crystallized two general systems—Pearson's system of closed curves and the Gram-Charlier open series. During the last two decades, however, there has been no material development from this angle except by way of appraising the comparative advantages[6] of these two major systems of specification.

By taking enough terms in the open Gram-Charlier series, we may theoretically secure a perfect fit to an observed distribution, but even a superficial knowledge of the fluctuations to be expected in a series of observed frequency distributions of samples of a given size taken under essentially the same conditions leads one to question the significance of closeness of fit thus obtained. It was not until 1900 that statisticians were given the necessary tool in the form of Pearson's χ^2 test to enable them to test the hypothesis that an observed distribution could have arisen from some specified universe with known parameters; not until

[5] Cf. Rietz, H. L., *Mathematical Statistics* (Chicago: The Open Court Publishing Company, 1927), Chap. III.

[6] One such interesting appraisal is that of J. F. Steffensen in *Some Recent Researches in The Theory of Statistics and Actuarial Science* (Camb. Univ. Press, 1931), pp. 35–48. He gives reasons for believing that Pearson's curves are preferable.

later that the χ^2 test was extended to allow for estimates of the parameters from a sample.

DISTRIBUTION

In order to test the hypothesis that one or more values of a variable taken under presumably the same essential conditions constitute a random sample from a postulated universe, it is necessary to have deduced the nature of the variation that may be expected from observation to observation, or from some function of a group of observations to the same function of another group, so that the observed may be compared with the theoretical variability. Now, it is customary to attach significance to both confirmation and infirmation as a basis for inference. However, there is little object in studying confirmatory evidence unless we can feel assured that we have succeeded in maintaining the same essential conditions and that the observed values are free from constant errors.

As an illustration, we may consider the measurement of an objective constant such as the charge on an electron. The literature certainly reveals the tendency of the physicist to emphasize first of all the need for gaining assurance that the observations are free from constant errors, and that the differences between observations are not significant. In fact, the physicist seldom tabulates more than five or ten observations after he has satisfied himself that he has maintained the same essential conditions and eliminated constant errors. Of course, Millikan in his original report on the measurement of the charge on an electron presented fifty-eight values, but his final estimate was based upon a comparatively small group because he felt that the others gave evidence of bias.

Since it is essential first to determine whether or not observed differences are significant, it is apparent that the technique of testing the hypothesis of constancy must be such that it can be used in testing significant differences between small groups of observations, if it is to be of much importance to the research man who realizes, as does the physicist, the lack of significance of multiplication of instances until assignable differences and constant errors have been eliminated, in so far as it is humanly possible to do so.

Keeping this situation in mind, we are in a position better to appreciate the practical significance of the developments in distribution theory taking place since 1900. Although prior to that time, statisticians were using various measures of central tendency, dispersion, skewness, kurtosis, and correlation, nevertheless comparatively little was known about the distributions of such measures for samples of any given size n drawn from even such a simple specified universe as

the normal distribution. Of course, the distribution of the averages of samples had been known for a long time prior to 1900; so also had the distribution of the standard deviations been discovered by Helmert in 1876, although this work was overlooked by most statisticians until some time after it was rediscovered empirically in 1908 by "Student," and rigorously in 1915 by R. A. Fisher. It remained for theoretical statisticians of the present century to deduce the distribution functions of various statistics as a function of sample size n, first under the assumption that the samples had been drawn at random from the normal law, and then to investigate the effects of functional forms of the universe upon the distribution of these statistics.

It should be emphasized that the object of such studies is not to remove the necessity of taking so-called large samples, but rather to put into the hands of the research man a useful tool for detecting significant differences applicable to small samples of observations which the careful scientist recognizes must be done before significance can be attached to large samples.[7]

Since 1900, exact distribution functions for any sample size for many statistics of samples drawn from a normal universe have appeared in rapid succession, largely at the hands of R. A. Fisher and his associates at the Rothamsted Experimental Station. Preliminary studies have been made, in particular by Rider and by Egon Pearson and some of his associates, to determine the effect of the functional form of the universe upon the distribution functions of some of the more important statistics. An excellent survey of much of this work up to 1930 has recently been given.[8] Similar reviews by Irwin for the years 1930 and 1931 have appeared in the *Journal of the Royal Statistical Society* for 1931 and 1932, respectively. Prospective students of this subject will find the extensive bibliographies of Rider and Irwin of great help. It may be of interest that Irwin lists 89 articles appearing in 1931.

Not only has marked progress been made in deducing exact distribution functions of many important statistics, but also in showing how many of these distribution functions are related to four compara-

[7] On this important point, Egon Pearson makes the following pertinent comment: "Critics of small sample theory are inclined to argue, 'What can you infer from two samples of five? Give us two samples of 100 and we may tell you something!' They do not realize that the distinction is often not of this kind at all. The comparison lies between a technique which can deal with *both* (a) 40 samples of 5 (or even 100 of 2), and (b) 2 samples of 100, and a technique which can only deal accurately with (b)." "A Survey of the Uses of Statistical Method in the Control and Standardization of the Quality of Manufactured Products," read before the Royal Statistical Society, December 20, 1932.

[8] Rider, Paul R., "A Survey of the Theory of Small Samples," *Annals of Mathematics*, Second Series, XXXI (October, 1930), 577–628.

tively simple distributions, namely, the normal law, Karl Pearson's χ^2, "Student's" ratio of the error of the average of a random sample drawn from a normal universe to the observed standard deviation, and R. A. Fisher's variable z, defined by the relation $e^{2z} = (s_1^2/s_2^2)$, where s_1^2 and s_2^2 are estimates of the variance of a normal population derived from two samples from that population. Largely to the work of R. A. Fisher do we owe this contribution, and as early as 1924 he gave the following table[9] to indicate the applications of the four chief cases of the z distribution:

I Normal curve	II χ^2	III Student's	IV z
Many statistics from large samples	Goodness of fit of frequencies Index of dispersion for Poisson and Binomial samples Variance of Normal samples	Mean Regression coefficient Comparison of means and regressions	Intraclass correlations Multiple correlation Comparison of variances Correlation ratio Goodness of fit of regressions

Since 1924, Fisher, Hotelling, Egon Pearson, McKay, and others have succeeded in relating certain other exact distributions to one or the other of these four.

The practical importance of being able to relate the exact distribution functions of many statistics to a comparatively small number of distributions is that it reduces the number of tables required for testing hypotheses in respect to various statistics. For example, it was pointed out by Fisher in 1924 that the distribution of the ratio of the observed variance s^2 for a sample of n drawn from a normal universe with standard deviation σ' to the square of the standard deviation σ' is distributed as χ^2, where χ^2 is put equal to $n_1 s^2/\sigma'^2$, and where n_1 is the number of degrees of freedom, being one less than the size n of the sample.

Thus, for example, we might ask the question: In a sample of sixteen drawn from a normal universe, what is the probability of occurrence of an observed standard deviation 20 per cent greater than the unknown true standard deviation of the parent population? Karl Pearson[10] solves this problem through the use of his tables giving indirectly the integral of the distribution of the standard deviation and obtains the probability 0.08329. We may, however, make use of the relationship of

[9] "On a Distribution Yielding the Error Functions of Several Well-known Statistics," presented at the International Mathematical Congress, Toronto, Canada, 1924. This paper outlines the framework around which Fisher's *Statistical Methods for Research Workers* has been built.

[10] *Tables for Statisticians and Biometricians*, Part II, page civ.

χ^2 to the ratio $n_1 s^2/\sigma'^2$ and through the use of the well-known χ^2, P tables obtain directly the probability 0.0834.

STATISTICAL INDUCTION

A. Estimation.—Assuming that a sample is a random one from a population of a given functional form but with unknown parameters, there arises the important practical problem of estimating the magnitudes of the parameters. Thus for the case of one variable specified by (1) where f' is assumed known but the λ''s are assumed to be unknown, we have the problem of estimating the λ''s from the available information Q. Now, of course, we may limit Q to the evidence given by the sample, or we may include in addition as a part of Q evidence available before the sample was taken.

In 1922, Fisher[11] proposed three criteria of estimation: (a) The criterion of consistency, which states that when applied to the whole population, the derived statistic should be equal to the parameter; (b) The criterion of efficiency, which states that in large samples, when the distributions of statistics tend to normality, that statistic should be chosen which has the least probable error; (c) The criterion of sufficiency, which states that the statistic chosen should summarize the whole of the relevant information supplied by the sample. This criterion of sufficiency is interpreted by Fisher as meaning that if λ_i' be the parameter to be estimated, Θ_{i1} a statistic which contains the whole of the information as to the value of λ_i' which the samples supplies, and Θ_{i2} any other statistic, then the surface of distribution of values Θ_{i1} and Θ_{i2}, for a given value of λ_i', is such that for a given value of Θ_{i1}, the distribution of Θ_{i2} does not involve λ_i'. The researches of Fisher have led him to the conclusion that an efficient statistic can, in all cases, be found by what he terms the method of maximum likelihood, assuming no other information than that the sample has been drawn at random from a population of given form but with unknown parameters.

When a sample is so large that it would be generally agreed by students of the subject that the information given by the sample overshadows all pertinent information available prior to the taking of the sample, these criteria, I believe, are generally accepted as constituting a rational basis for deciding upon estimates of parameters. On the other hand, as previously noted, it is generally agreed by students of induction that any induction or judgment should be consistent with all available pertinent information. Certainly, in the case of small samples, this generally recognized criterion must be considered in conjunction

[11] *Trans. Roy. Soc. of London*, Series A, Vol. CCXXII, 309–368.

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with those for the consistency, efficiency, and sufficiency, of an estimate based upon a sample. Recent discussions bearing upon this general subject have been given by Molina and Wilkinson,[12] Karl Pearson,[13] and others. In fact, Pearson introduces in contrast with the "most likely" estimate based solely upon a sample, what he calls the "most reasonable" estimate which takes into account prior information. In this connection, Pearson argues that in almost all cases there is some, even though vague, *a priori* experience existing that should be used, together with the information given by the sample, in finding what he calls a most reasonable estimate.

This argument certainly has weight, because in a practical case we seldom, if ever, know *a priori* that a sample has been drawn at random from a universe of given functional form, and it usually works out in such instances that before a practical man is willing to believe that the given method of measurement will lead to the sought-for objective value as a statistical limit simply through increasing the size of the sample, he has already formed some judgment that the objective value lies within some agreed-upon range and experienced what he considers a rational degree of belief in this judgment that must certainly be considered when interpreting a small sample.

In any case, however, a knowledge of the distribution of a given statistic as a function of sample size enables the experimentalist to determine the relative significance of increasing the number of observations under statistically controlled conditions, and shows that, at least for efficient statistics, the degree of precision attained is inversely proportional to the number of observations. Furthermore, a knowledge of the distribution in any given case enables one to appreciate the significance of the variability introduced in the process of sampling. As an illustration, suppose we are given a sample of four known to be drawn from a normal universe of unknown average $\overline{X}'$ and standard deviation σ'. Based upon this sample, what shall we take as estimates of $\overline{X}'$ and σ'? The knowledge that for all practical purposes the observed standard deviation for a sample of four may vary all the way from zero to more than twice the true standard deviation σ' must of necessity make one cautious in placing too much reliance on any estimate based solely upon a sample of four.

B. Testing hypotheses.—Given a random sample Σ, an important problem is that of determining whether or not it is likely to have come from a certain population π which may be either completely or par-

[12] "The Frequency Distribution of the Unknown Mean of a Sampled Universe," *Bell System Tech. Jour.*, VIII (Oct. 1929), 632–645.

[13] *Loc. cit.*, pp. clxxi–clxxx.

tially specified. Two distinct methods of approach are open: (a) To start from the population π and seek the probability that a sample Σ should have been drawn therefrom; (b) To start from Σ and seek the probability that π is the population sampled. Two recent papers[14] by Neyman and Egon Pearson break new ground in providing a critical basis for developing and using statistical criteria. In treating the first of these two methods they note two important requirements: (1) We must be able to reduce the chance of rejecting a true hypothesis to as low a value as desired, (2) The test must be so devised that it will reject the hypothesis tested when it is likely to be false. Pearson and Neyman were the first to emphasize the latter point and to provide a criterion or test which takes both into consideration.

In two later papers[15] they extend their studies to the problems of two and k samples. In the case of two samples they fix upon the set of admissible hypotheses concerning the populations π_1 and π_2 from which two samples Σ_1 and Σ_2 have been drawn, restricting this by the further assumption that π_1 and π_2 are both normal. They develop a test for the hypothesis H that π_1 and π_2 are identical. They then show how R. A. Fisher's z test may be used in testing the hypothesis H_1 that the two samples Σ_1 and Σ_2 have come from unknown normal populations with the same variance but with means having any values whatsoever. Similarly, they show that if it be assumed that the populations π_1 and π_2, besides being normal, have the same variance, the "Student" t test is the appropriate one for testing the hypothesis H_2 that the means of these populations are the same. They then argue that their criterion for testing hypothesis H is more crucial than tests of H_1 or H_2 taken separately although they point out that in many problems the hypothesis to be tested will present itself in one of the two forms, either H_1 or H_2.

These results are of fundamental importance in testing for assignable causes of variation or lack of homogeneity, which tests should always be made before one places too much significance on the contributions of a single sample no matter how large. The importance of this work, however, is not alone in the new criteria provided, but also in the fact that these papers present an excellent critical introduction to some of the philosophical and logical, as well as the mathematical, problems involved in developing rational techniques for testing hypotheses.

[14] "On the Use and Interpretation of Certain Test Criteria for Purposes of Statistical Inference. Parts I and II," *Biometrika* XXA (July and December 1928), 175–240 and 263–294.

[15] "On the Problem of Two Samples" and "On the Problem of k Samples," *Bull. de l'Académie Polonaise des Sciences et des Lettres* 1930, pp. 73–96, and 1931, pp. 459–481.

In trying to appraise contributions in statistical technique, we must keep in mind that the ultimate judgment or inference cannot be stated with certainty, and that even though we were in a place to give an accurate estimate of the degree of rational belief p_b' associated with a given judgment, this degree of rational belief is based upon certain evidence Q. Anyone passing upon the significance to be attached to a judgment based upon a given set of data must, of course, attempt to *weigh*[16] the significance of each of the contributory quantitative and qualitative elements going to make up the pertinent information Q.

Some Important Books on Technique, 1931–32

1. *Tables for Statisticians and Biometricians, Part II*, edited by Karl Pearson. London: Cambridge University Press, 1931. 512 pages. England 33*s*, export $7.30.

   Contains not only the tables issued in *Biometrika* during the past seventeen years, but also 250 pages of valuable and critical discussion of the use of these tables.

2. *The Methods of Statistics*, by L. H. C. Tippett. London: Williams and Norgate, Ltd., 1931. 222 pages. 15*s*.

   An excellent introduction to many of the recent developments in statistical technique made possible through developments in distribution theory during the past three decades.

3. *Contributions to the History of Statistics*, by Harold Westergaard. London: P. S. King & Son, 1932. vii+280 pages. 12*s*. 6*d*.

   Valuable as a survey of the evolution of statistical technique up to 1900.

4. *Statistical Methods for Research Workers*, by R. A. Fisher. 4th edition. London: Oliver & Boyd, and New York: G. E. Stechert & Co., 1932. xi+307 pp. 15*s*.

   Revised and enlarged edition, bringing up to date the survey of R. A. Fisher's major contributions to statistical technique from 1912 to 1932.

5. *An Introduction to the Theory of Statistics*, by G. U. Yule. 10th edition. London: Charles Griffin and Co., Ltd., and Philadelphia: J. B. Lippincott Company, 1932. xv+434 pages. 12*s*. 6*d*.

   Principal item of revision is that of bringing the excellent bibliography in this classic up to date.

[16] Cf. Keynes, J. M., *A Treatise on Probability* (New York: Macmillan Co., 1921), p. 77 *et seq*.

6. *Business Statistics*, by Riggleman and Frisbee. New York and London: McGraw Hill Book Company, Inc., 1932. xix+707 pages. $4.00.

Part I gives an interesting introduction to statistical methods in respect to techniques of gathering, tabulating, and presenting data, and calculating averages, index numbers, dispersions, and other simple statistics, in so far as this can be done without giving due consideration to the developments of the present century as here reviewed in respect to the problems of specification, distribution, and induction. Part II, comprising approximately one-half of the book, constitutes an excellent introduction to some of the complex business and economic problems faced by the industrialist.

The student of statistical technique will doubtless find it interesting to start with Westergaard's history, review Yule's interesting summary and the practical discussions of applied technique in the field of business as presented by Riggleman and Frisbee. He will note the emphasis on techniques of collecting and presenting data, and those of calculating certain characteristics of observed data. If now he studies the above-mentioned texts by Pearson, Fisher, and Tippett, he will experience a definite change in emphasis and will begin to appreciate that the rational interpretation of data depends not only upon a knowledge of the statistical techniques involved in testing statistical hypotheses, but also that the techniques of collecting and presenting data inherently depend upon the techniques and the hypotheses that are going to be used in the interpretation thereof.

Bell Telephone Laboratories

TIME SERIES: THEIR ANALYSIS BY SUCCESSIVE SMOOTHINGS

By Lewis A. Maverick

Introduction

The search for an ideal type of trend line is intriguing but baffling. Many have advocated the S curves: the logistic, the Gompertz, and the arc-tangent. But it has been suggested by Frisch[1] that the highest or simplest trend attainable still contains a significant cyclical or periodic element, and Sombart[2] posits a fundamental periodicity in economic history.

These suggestions leave us in some doubt whether the fundamental type of trend is the non-recurrent logistic or a periodic function of the type of a sine curve. There is sufficient doubt between the two type forms that neither can, without challenge, be pronounced canonical. This doubt justifies an attitude of eclecticism, and an empirical rather than a rigid mathematical procedure.

Practically, it is difficult to fit a sine curve or other periodic function, so that if the problem is attacked as one of fitting a mathematical trend, it narrows down to the non-recurrent curves. Of the non-recurrent trends, the most suitable are probably the S curves, and of these the logistic may be considered representative. It may be applied as a simple S curve to a cumulative variable affected by a single major stimulus, or in more complex generalized forms. However, the generalized logistic can be fitted only by trial and error, a tedious process.

The empirical method presented below accomplishes much the same result with simplicity and ease, and is offered as a practical substitute. Even though the statistician declines to use an empirical curve as the trend, and plans finally to fit a formal logistic (or other curve), he will be aided by going first through the rapid empirical procedure, to crystallize his ideas as to the shape to be sought in the formal curve.

The subject of empirical curves may be introduced with moving averages. These may be constructed from formulas of varying complexity.[3] The great shortcoming of the moving functions is the rigidity of the period, which, even though an elaborately weighted moving function is employed, prevents the resulting curve from smoothing out

[1] Ragnar Frisch, *Analysis of Statistical Times Series*, New York, April, 1927, pp. 121 (mimeographed).

[2] Werner Sombart, "Economic Theory and Economic History," *The Economic History Review*, II, (January, 1929). 1–19.

[3] Frederick R. Macaulay, *The Smoothing of Time Series*, New York: National Bureau of Economic Research, 1931.

fluctuations as a satisfactory trend must do. In fact, because of the varying periods in the actual data and the rigid period in the moving function, the method sometimes actually introduces spurious fluctuations in the trend or smoothing curve. Yet the moving average may be profitably used in the lesser capacity of a guide to the location of the trend, provided that the period comprised in the average is altered freely from cycle to cycle. Wardwell has demonstrated the feasibility of employing a moving average of period which changes systematically with the length of the successive cycles.[4] The moving average has the property of cutting off from the original curve substantially equal positive and negative areas. On arithmetic paper the arithmetic average has this property; on semi-logarithmic paper, the geometric average.

This paper presents a different type of empirical procedure; the trend is located graphically instead of arithmetically. The graphical method succeeds in effecting the smoothing that is attained but awkwardly by the moving average.

The principle has been generally accepted that the most accurate way of fitting a trend is by the method of least squares. However, there are times when the limited segment of material that can be obtained seems to contain incomplete cycles. For example, the curve may show two peaks and one trough; in such case it may well be inferred that a straight line trend fitted by least squares will later be found to be located too high, if additional data are secured to complete the cycles. Even if the cycles are complete, if the dates of beginning and ending the series are other than at peaks (or at troughs) a false slope will be given the fitted trend.

Successive Smoothing Lines, Fitted Graphically

If the original data are plotted on a time chart, inspection will reveal in most cases a complex movement containing elements of unlike amplitude and period. If one is willing to regard a period as "regular" when its recurrences are of approximately the same length, and if one is willing to use empirical methods and free-hand smoothing lines, concealed regularities can be found in the apparent irregularities of movement in the chart. Frisch has demonstrated the feasibility of such a graphical procedure.[5] The method followed here is patterned after his work, although his criterion of points of inflection is supplemented by several others.

The work proceeds from the known, by stages, to the unknown,

[4] Charles A. R. Wardwell, *An Investigation of Economic Data for Major Cycles*, Philadelphia, 1927. (Not available at the publisher's; obtained from the author at Northwestern University.)

[5] *Op. cit.*

from monthly data to a smoothing line of the first order from which seasonal fluctuations have been eliminated, from this line to a second smoothing line from which the three-year cycle has been eliminated, from this, if there be forty or fifty years' information, to a third smoothing line from which the fifteen or twenty year cycle has been eliminated, and, if the data warrant, to still further stages. Each successive smoothing line is markedly simpler, and reveals more basic fluctuations. If the series is so long as to permit the location of trends of high orders, it is very likely that fundamental changes will have occurred either in the variable itself or in important conditioning factors. This probability gives still further warrant to the empirical procedure.

To locate the first smoothing line, from which the seasonal fluctuation is eliminated, several guides or criteria may be employed.

One is the twelve months' moving average. The moving average carries with it the characteristic of setting off approximately equal positive and negative areas. If the original data had been "pure" in the sense that they followed some mathematical function, then probably the aid of the moving average might be dispensed with. But the data in economic series are not pure, and therefore the moving average with its stable position gives valuable aid, but should not be trusted without further scrutiny.

Another guide will be found in a free-hand curve drawn through the points of inflection in the original curve, smoothed graphically. To prepare for this free-hand smoothing line, the chart of the monthly figures should be examined; if this curve is found to have a single peak each year and to be reasonably regular from month to month, it may prove feasible to locate directly its points of inflection. If, however, the curve shows more than one peak per year, or if there are many irregularities, a three-months' moving average may be applied, and if necessary reiterated in portions of the curve, until single annual peaks and reasonably smooth curvature have been secured. Then the points of inflection should be marked in the smoothed curve, to serve as guides to the desired smoothing line. This line may now be drawn, passing very close to the points of inflection; but, because of the irregular nature of the original series, the line should concede a little one way and the other, as dictated by two criteria: (a) sharp curvature should be avoided; and (b) the curve should be drawn roughly midway between the upper and lower "bounds" (two enveloping free-hand curves drawn to touch the annual peaks and troughs respectively, as indicated in Fig. 1).

Points of inflection are never at maximum or minimum positions, and consequently the line connecting them has an intermediate location that is essential in a trend. This line has, in addition, the interesting

property, which has been pointed out by Frisch, that it is the smoothest possible locus of points toward which the original (or "lower order") curve is concave. An object moving along the curve will find its motion continuously accelerated toward this smoothing line. This line is thus the gravitational axis of the original curve, a property that makes it representative of that curve in a unique sense.

The criterion of smoothness and gentle curvature appeals to the intuition as "reasonable." However, exceptional cases may make it necessary that less weight be given the criterion of smoothness. Concessions from smoothness must be made in dealing with variables which are as sensitive as stock prices, or in accommodating to the effect on San Francisco's commerce of the earthquake and fire of 1906, or of the opening of the Panama Canal.

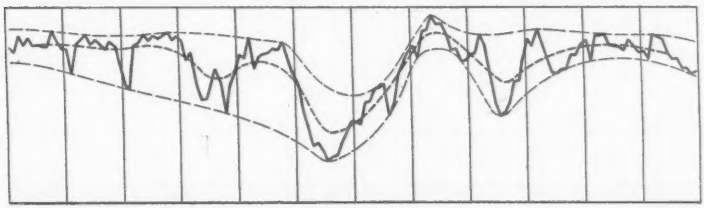


FIG. 1

A compromise between the twelve months' moving average and the free hand line (based on inflection points and upper and lower envelopes) give the final smoothing line of the first order.

The two bounds serve chiefly as aids to the organization of thought. The statistician inspects the original curve and marks the peaks which he regards as significant for this stage of the analysis. He draws the upper bound to touch them. A similar line touching the troughs is the lower bound. He roughs in his approximation to the trend midway between the upper and lower bounds, and then turns back to study the original curve. At this point he is likely to find it advisable to change his list of "approved" peaks and troughs, to add to his list some he had earlier thought too petty, and to drop others as probably of more significance to another stage of the analysis. This device enables him to achieve consistency in the matter of complexity throughout a long series, to the end that each "order" of fluctuation shall be homogeneous.

After the smoothing line of the first order has been located, it serves as the base for the location of the smoothing line of the second order. To locate smoothing line II, it is necessary to eliminate from smoothing

line I the shortest discernible cycle, which usually proves to be three or four years in length. As in the previous construction, the moving average will have some merit as a guide, but since the length of the fluctuation in smoothing line I varies (as was not the case in the annual cycle), the period of the average should be arbitrarily altered from cycle to cycle. In practice, there may be substituted for the moving average a free-hand curve drawn according to the principle of equal areas. As in the previous case, the two curves will need to be reconciled.

In similar fashion, smoothing line III may be located by eliminating the shortest discernible cycle remaining in smoothing line II (probably

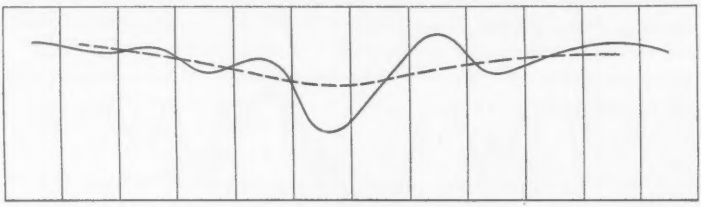


FIG. 2

from seven to twenty years in length); and smoothing line IV to eliminate the shortest remaining fluctuation from smoothing line III. If the series is long enough, lines of even higher orders may be located.

If the statistician is in doubt between two competing locations for one of the smoothing lines, he may apply the established criteria for goodness of fit, of which that of minimum standard deviation is of principal value. He may take comfort, in such a quandary, in the fact that whichever location he accepts, no fluctuations will be ignored in his analysis; the issue is merely whether a particular fluctuation is to be emphasized in the lower order analysis or that of higher order.

When the several orders of smoothing lines have been located, a description may be written of each, recounting the dates and ordinates of peaks and troughs, and of intersections with the line of next higher order, the slopes and curvatures between these points, etc.

THE SEVERAL ORDERS OF FLUCTUATION

Royce[6] has expounded the principle that if the values of a variable extend over a wide range, there will be little likelihood that the distribution will be smooth and unimodal, but rather a tendency to assimilation into types or groups, that is, to a number of minor points of

[6] Josiah Royce, "The Mechanical, the Historical, and the Statistical," *Science*, XXXIX, no. 1007 (April 17, 1914), 551–566. Section IX.

concentration, separated by intervals in which the variable will be found with relative infrequency. He calls attention to the aid to fruitful thinking to be obtained from a recognition of this grouping into natural classes.

In applying this principle to time series, it may be noted that some forces, weak in strength or brief in application, act upon the variable so as to cause very slight fluctuations. From this minimum strength the forces increase in intensity, as shown in the increased amplitude and duration of the fluctuations they cause. According to Royce, it may be expected that these forces (and the resulting fluctuations) will fall into natural classes according to their strength (that is, according to the amplitude and duration of the fluctuations they cause), with reasonably clear demarcation between classes. The task of separating the fluctuations into "natural" classes has been accomplished by the several orders of smoothing lines.

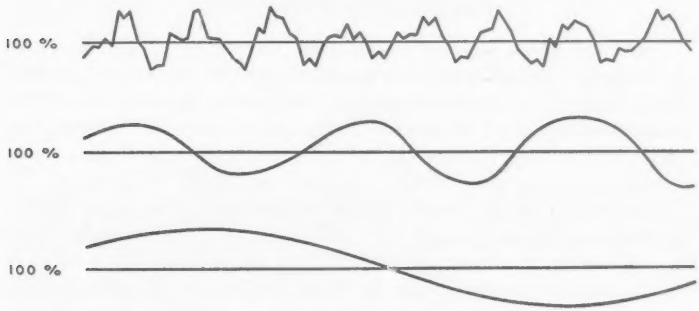


FIG. 3. (Top) Fluctuation of actual data about line I (not based upon Figs. 1 and 2).
(Middle) Fluctuation of line I about line II.
(Bottom) Fluctuation of line II about line III.

The system of smoothing here used makes practicable the separation of the two concepts of time commonly met in economic theory, the long period and the short period. Cause and effect, or other relationship, may now be studied separately in the two time fields.

One advantage of the proposed method of analysis of time series over the conventional procedure of throwing all fluctuations other than the fixed seasonal pattern into a single omnibus "cycle" is that several *orders of regularities* are made accessible, and that even the residual irregularities and random movements are assembled into homogeneous orders or classes for study. This has been pointed out by Frisch and Wardwell.

Charts exhibiting the components may be useful. For each order of fluctuation, a horizontal base-line with a value of 100 per cent may be drawn to represent the smoothing line from which the fluctuation is measured, and the percentage ratio of the line of next lower order to this line may be plotted.

An inspection of the charts of fluctuations will reveal an outstanding feature of the method of points of inflection; namely, that each order of fluctuation stands alone, homogeneous and simple, and this without sacrificing the complexity of reality, as the very number of curves evidences.

For each order of fluctuation, the chart of fluctuations may of course be reduced to a frequency table of deviations from "trend," and from each of these tables there may be calculated the standard deviation, the typical period, and other parameters, for the particular order of fluctuation.

Application to Forecasting

There are three general methods of forecasting in common use: (1) carrying forward somewhat mechanically the typical or standard fluctuation; (2) cross-section analysis, estimating the resultant of the probable effects of all discernible forces acting upon the observed variable; and (3) historical analogy, in which a single former fluctuation is selected for comparison rather than the "standard" fluctuation.

The method herein presented falls in the first of the three classes. However, as will be pointed out, wherever any line of thought leads the statistician to expect a deviation from the standard pattern, it is readily possible for him to alter or shape the forward projection of the standard pattern, thereby admitting inferences based on methods (2) and (3).

In analysis, the order of procedure was from monthly data to line I, to II, to III, etc. In forecasting (see Fig. 4), the order of procedure is reversed. First, line III, if this be the highest order trend the analysis has located, may be projected forward from its terminal point (this, because of the uncertainty of bringing a moving average or a trend abreast of the current measurements, will usually be short by several years of the current date), continuing approximately, and in broad sweep, its recent slope and curvature. This curve should be modified according to inferences as to the long time movement, based upon cross-section analysis and historical analogy. Second, with the aid of the parameters obtained in the study of the fluctuation of line II about line III, line II may be carried forward from its terminal point, as a fluctuation about the projected line III for approximately a half-cycle. This projected line should be corrected, if historical analogy or cross-

section analysis gives reason to expect a departure from the standard pattern. Third, in similar fashion, a standard half-cycle of line I may be carried forward as a fluctuation about the projected line II. Fourth, the standard seasonal pattern may be imposed upon line I throughout its projected length (since the period of this fluctuation is constant, it is not necessary to restrict this forecasting line to a half-cycle of the seasonal movement).

The projected line III should be accepted as the forecast for dates eight to fifteen years in the future; line II for two to eight years in the future; and the "normal" curve (seasonal pattern superposed on line I) for dates less than two years in the future. Each of these forecasting

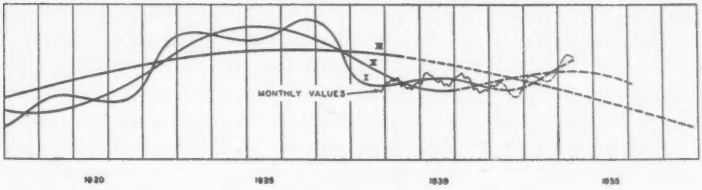


FIG. 4

lines may be refined to two further stages; first into a band of constant width of, say, two standard deviations (the value of the variable at any assigned future date may be expected to fall within this band); and further into an expanding funnel, indicating that for times more distant the precision of the forecast is diminished.

APPLICATION TO COMPARISON OF TIME SERIES

If two (or more) time series have been analyzed in the proposed fashion and the description of smoothing lines and of fluctuations has been arranged systematically, an extensive comparison becomes possible.

The first order comparison will indicate the relationship between the seasonal movements in the two (or more) series, and will give a much richer comparison than may be obtained by simply placing in juxtaposition and inspecting the two (or more) standard seasonal patterns. The second order comparison will indicate the relationship in the three-year cycle; the third order comparison the relationship in the major cycle (of about fifteen years' period); and the fourth, the relationship in a longer fluctuation, of thirty or forty years' period.

The correlations of the second and third orders will prove particularly meaningful. It is quite possible to find low correlation in one

order and high in the other. The study of causation and of correlation in these two orders of fluctuation will make possible a more effective explanation of business cycles. The theories as to the causation of "the" business cycle may be sorted into groups as they are found to apply to the long or the short movement.

Application to the Combination of Series into Index Numbers

From a group of time series which have been analyzed according to this system, certain series may be selected for inclusion in an index number to be constructed for a particular order of fluctuation, say the three-year cycle. The criteria for the selection will not be discussed here; on the whole the chief criterion is that the chosen variable show the particular order of fluctuation clearly. From each of the selected series, the desired order of fluctuation should be taken, in the form of units of standard deviation. The several series should then be combined into a weighted average; if desired, lagging may be employed.

In composing an index number for the fluctuation of next higher order, it is likely that a different selection of variables will be made, and the weights altered. The production of steel ingots may prove to be of more significance for the three-year cycle than for the fifteen, and real estate activity of more significance for the latter.

Summary

The procedure that has been outlined rests essentially upon the graphical location of a succession of smoothing lines by a controlled free-hand technique, each line eliminating the shortest fluctuation remaining in the line of next lower order. Organized description of the fluctuations is made possible as a result of their separation into orders by the agency of these smoothing lines. The richness of detail in the description is appropriate to the complexity of social and economic variables, and the details prove helpful not only in describing the series, but also in extrapolation, and in effecting comparison and combination with other series.

University of California at Los Angeles

THE NOTIONS OF HORIZON AND EXPECTANCY IN DYNAMIC ECONOMICS[1]

By J. TINBERGEN

IN A THEORY of economic dynamics, the ophelimity function of individuals must be supposed to depend on the quantities of goods consumed and the sacrifices brought, not only at the moment considered, but also at later moments. Their offer and demand schemes for each moment then depend not only on the prices governing at that moment, but also on the *price expectances* the individuals have for the future. Among those expectances, those relating to the near future will be of more importance than those relating to a further period. As a first approximation it might be supposed that only the expectances relating to a certain time period (the "*horizon*") are of importance, and all of the same importance. That means that the subject is at every moment t making a definite plan for the period from t to $t+\tau$, and then realizes certain parts of that plan. Before other parts could be realized, the subject makes a "revision" at the moment $t+1$, say, for the period from $t+1$ to $t+\tau+1$, etc.

The purpose of the present paper is to discuss, with the help of these notions, some results of statistical analysis, which cannot be explained by static theory, and which seem to teach something about horizon or expectances.

I

RELATION BETWEEN TOTAL ANNUAL SUPPLY OF NON-PERISHABLE CROPS AND AMOUNTS HANDLED

The amount handled is always smaller than the total supply (crop plus carry-over), the difference being the next year's carry-over. The static theory does not yield us a principle to calculate the proportion of sellings to total supply. I suggested a very much simplified scheme —in which the above-mentioned notions appear—to solve the problem, in *Zeitschrift für Nationalökonomie*, III (1931), p. 169.

In this paper I assumed that

1. demand has the form

$$v_t = f(p_t) + \alpha t$$

p_t being the average price during year t, α a constant;

2. cost of carrying over may be neglected;

[1] I am indebted to Prof. Th. Limperg, of the University of Amsterdam, for several critical remarks, and to the Netherlands' Central Bureau of Statistics for lending the clichés.

3. expectances of further crops and demand may be "reasonable," which means that (A) crop expectances are supposed to be equal to the average crop falling on the acreage and to increase every year with α; (B) demand expectance is supposed to be in the same relation with price expectance as actual demand with actual price.

I reached the conclusion that[2]

$$v_t = \bar{e}_t + \frac{1}{\tau}(E_t - \bar{e}_t) \tag{1}$$

$\bar{e}_t$ being the normal (average) drop for each year; E_t being the actual supply at beginning of crop year; τ being an average horizon of sellers.

This result may be generalized by assuming that apart from the carry-over considered in the above formula and the paper mentioned, there exists a certain stock to meet with the risks of unexpected changes; this supply may be supposed to be proportional to $\bar{e}$,and so the total real supply measured will be

$$s_t = E_t + \beta\bar{e}_t. \tag{2}$$

Formula (1) now turns into:

$$v_t = \bar{e}_t + \frac{1}{\tau}[s_t - \beta\bar{e}_t - \bar{e}_t] = \gamma\bar{e}_t + \frac{1}{\tau}s_t. \tag{3}$$

It is possible to test statistically this relation and to determine τ, as soon as a series of annual figures on v_t and s_t are available—$\bar{e}_t$ assumed to be equal to the trend value of v_t. Such data are indeed available for several important crops; those for coffee, cotton, wheat, and sugar, are given in Tables I and II. From the theoretical deduction it is clear that as supply we shall have to consider the sum of crop and carry-over as far as the latter is in the possession of producers and of dealers. Stocks held by consumers should not be taken account of. It is, however, difficult to tell, and perhaps interesting to discuss at a later opportunity, whether, for instance, importers' stocks should be included or not. When the situation is such that the great bulk of stocks is held by importers, as is the case for coffee before the war, it is clear that those stocks should be included; for our deduction relates especially to those economic subjects that bear the risk of carrying over. The figures for coffee given in Table I therefore include those stocks. The figures for wheat and cotton do not include them; for

[2] As other results of this analysis, one finds two relations that are in good accordance with facts, viz., (1) equality of prices and price expectances for next year except in case of a very short crop; (2) inverse correlation between average price during crop year and carry-over at end of crop year.

TABLE I
TOTAL SUPPLY AND DELIVERIES OF COFFEE, 1884–1913 AND 1920–1931
(Millions of Sacks)

Year	v_t a. World Deliveries	s_t b. Total Supply	c_t c. Trend of Deliveries	C in % of c	a in % of c
1884	9.4	14.8	8.7	170	108
1885	10.6	15.7	9.0	174	118
1886	10.1	14.1	9.3	152	109
1887	10.0	14.3	9.6	149	104
1888	8.1	11.4	9.9	115	82
1889	9.2	12.1	10.2	119	90
1890	9.4	12.2	10.5	116	89
1891	8.7	11.1	10.8	103	81
1892	10.8	13.5	11.1	122	97
1893	10.9	14.0	11.4	125	96
1894	10.6	13.0	11.7	111	91
1895	11.2	13.9	12.0	116	93
1896	11.1	14.7	12.3	120	90
1897	12.2	16.2	12.6	129	97
1898	14.6	21.0	12.9	165	113
1899	13.5	20.1	13.2	152	102
1900	14.0	21.6	13.5	160	104
1901	14.3	25.2	13.8	183	104
1902	15.9	28.7	14.1	204	110
1903	16.0	29.8	14.4	207	111
1904	16.1	29.7	14.7	202	109
1905	16.2	28.8	15.0	192	108
1906	16.7	30.5	15.3	200	109
1907	17.5	34.2	15.6	219	112
1908	17.5	33.3	15.9	210	110
1909	18.6	35.2	16.2	217	115
1910	18.2	32.4	16.5	197	110
1911	17.2	30.8	16.8	184	102
1912	17.5	30.9	17.1	180	102
1913	17.1	30.8	17.4	177	98
1920	14.4	23.1	17.5	132	82
1921	18.1	27.5	18.0	153	100
1922	17.9	25.9	18.5	140	97
1923	19.8	24.3	19.0	128	104
1924	20.7	30.7	19.5	158	106
1925	19.0	28.5	20.0	142	95
1926	20.6	31.5	20.5	154	100
1927	21.6	39.9	21.0	190	103
1928	21.8	40.5	21.5	188	102
1929	21.7	46.7	22.0	212	99
1930	22.6	51.9	22.5	230	100
1931	23.8	58.2	23.0	254	104

Sources and Method of Calculation: World Deliveries from: Roth, *Die Übererzeugung der Welthandelsware Kaffee, 1790–1929*. Total Supply calculated by adding Stock at end of year to World Deliveries during year. Trend of Deliveries: free-hand straight line trend. Even rather important variations in constants of Trend do not affect materially the result for our purposes.

wheat this makes only a small difference in the result for τ; for cotton, however, the difference is of more importance; and τ is larger when importers' stocks are included.

TABLE II
SUPPLY AND DISAPPEARANCE, WHEAT, AMERICAN COTTON, AND SUGAR, 1921–1931

Crop year beginning	Wheat		American Cotton		Sugar	
	Supply, million bushels	Disappearance	Supply, million bales	Disappearance	Supply, million tons	Disappearance
1921	3485	3164	13.28	10.78	20.0	18.8
1922	3477	3112	12.26	11.33	19.6	18.3
1923	3867	3449	11.10	10.30	21.4	20.4
1924	3499	3184	14.44	13.72	24.7	23.1
1925	3654	3315	16.84	14.44	26.2	23.6
1926	3759	3382	20.15	17.80	26.3	23.9
1927	3977	3548	15.13	13.64	27.7	25.2
1928	4340	3676	15.79	14.59	29.6	26.8
1929	4095	3459	15.75	12.48	29.6	25.4
1930	4435	3674	17.03	11.69		
1931	4465					

Sources and method of calculation: *Wheat:* Wheat Studies of the Food Research Institute, July 1932, p. 422. From the supply, however, stocks in hands of European importers have been deducted, and to disappearance has been added the increase of those stocks. *Am. Cotton:* Bulletins on Cotton Production and Distribution of the Department of Commerce. Supplies are calculated from crops by adding stocks in public storage in the U. S. and stocks elsewhere in the U.S.; disappearances by subtracting from crops the increases in those stocks. 1921 and 1922 figures approximated from other sources. *Sugar:* "Studies in the Artificial Control of Raw Materials: Sugar"; by J. W. F. Rowe, *London and Cambridge Economic Service.*

To calculate τ it is necessary to make some assumption about $\bar{e}$. As already mentioned above, I have supposed $\bar{e}$ to be equal to the trend value of v_t. In the case of coffee a straight line has been taken as trend, drawn by hand, as it was of no importance to reach great accuracy. The slope of the trend for the pre-war period was chosen as 0.3 millions of bales, for the post-war period as 0.5. (See Table I.) To eliminate further long-run influences, v_t as well as s_t were calculated as percentages of the trend. From Fig. 1 it is seen that there exists a fairly good correlation between those two trend percentages, which enables us to evaluate τ. There seem to be three distinctly different periods in any of which τ has a different value. By rough graphical calculation—a more accurate method seems to have no significance—I find:

for the period 1884–1898: about 2 years.

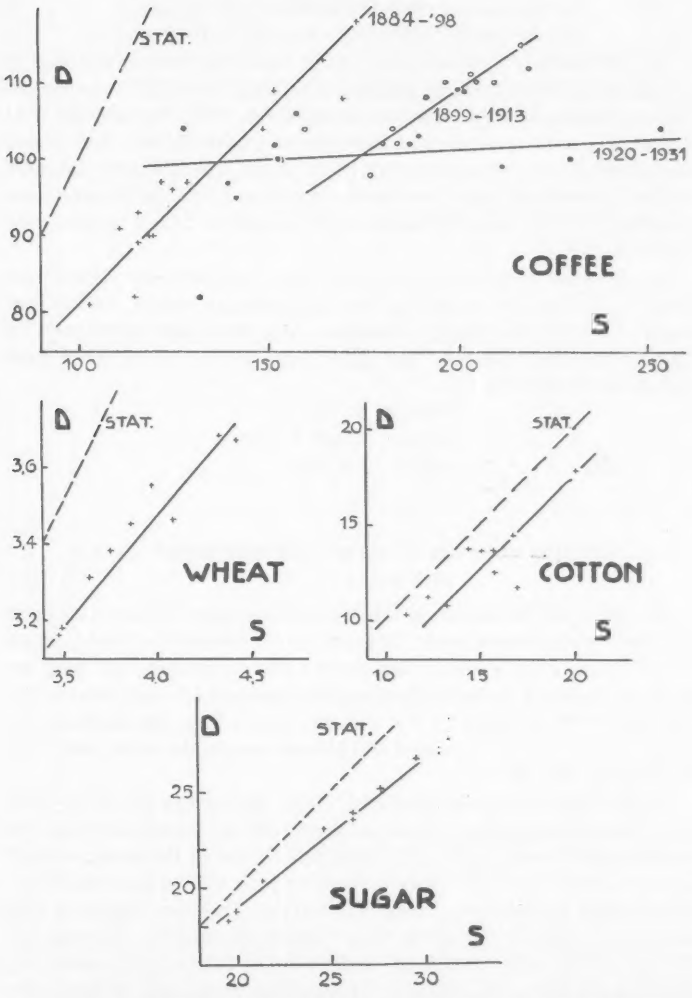


FIGURE 1.—Relation between supply (=carry-over+crop) S and disappearance D during crop year. Coffee: 1884–1913 and 1920–1931; wheat: 1921–1931, cotton: 1921–1930 and sugar: 1921–1929. For data see Tables I and II. The dotted line "Stat." indicates the relation demanded by static theory without taking account of "risk-stocks"; when those latter are included, the direction of the dotted lines still remains characteristic for the "static relation."

for the period 1898–1914: about 3 to 4 years.
for the period 1921–1931: more than 10 years.

It is interesting to remark that Roth[3] indicates 1898 as the year in which the period of market equilibrium existing since 1887 was changed into a period of overproduction lasting until 1908. We can add that before the war a considerable decrease of stocks did not take place; and after the war, especially after 1925, overproduction grew still more serious. Our results may therefore be formulated: that in times of overproduction sellers in a still higher degree behave as if their horizon were widening.

For the cases of wheat, cotton, and sugar, only post-war figures were studied. It was not necessary here to eliminate trends, and it was supposed that $\bar{\varepsilon}$ was simply a constant. Also, here there seems need for further detailed research. The provisional results found for τ were (cf. Table II and Fig. 1):

wheat: 1.7 years,
cotton: about 1 year,
sugar: 1.3 years.

II

RELATION BETWEEN DIVIDEND PAID ON CERTAIN STOCKS AND THEIR "WORTH"

By worth will be meant, in this paper, the product of a stock's price and the yield of state bonds. This product indicates the fixed interest to which the stock's yield is supposed to be equivalent. The relation between dividend really paid at a given moment (D) and worth (W) can only be established by static theory in the case the absolute certainty exists that the dividend will always remain the same; and then it is simply: $D = W$.

In the case of variable dividend, static theory cannot be applied. As a first approximation we can assume now, in accordance with the notions introduced above, that marginal buyers in the stock market have a definite horizon τ; then a stock's worth will be determined by the dividend expectances during this horizon. This approximation will hold true especially for other than "highly speculative" buyers; for the latter will take account of, and probably take most account of, price expectance for the moment of re-selling. Supposing we have not to do with such speculative stocks, the question arises on what factors dividend expectancies will depend. Indications to the answer are given in the statistical material represented in Charts 2–6 and Tables III–VI.

[3] H. Roth, *Die Übererzeugung in der Welthandelsware Kaffee im Zeitraum von 1790–1929*, Jena, 1929.

TABLE III

DIVIDEND (D) PAID IN THE YEARS 1921–1932 AND WORTH (W) OF THE STOCKS IN QUESTION IN THE QUARTERS (QU) OF PAYMENT

Enterprises	Qu.	1921	1922	1923	1924	1925	1926	1927	1928	1929	1930	1931	1932
1. Werkspoor.....D		8	8	6	5.5	6.6	6.6	6	6	7	7.5	7	0
W	2	4.8	3.9	4.2	4.3	4.0	4.0	4.4	4.0	4.2	4.5	3.6	1.3
2. Ned. Scheepsbouw Mij.D		8	8	5	0	0	0	0	0	5	6	6	0
W	2	4.6	3.5	3.7	3.3	2.0	1.9	3.6	3.7	3.9	3.5	2.7	1.4
3. Scholtens A'meelfabr..D		0	0	0	4	4	0	0	0	0	0	0	0
W	4	[1]6.1	3.5	3.9	4.3	4.3	3.4	3.3	2.4	2.3	1.4	0.7	1.5
4. Stork.....D		11	7	5	0	6	6	6	6	5.5	0	0	
W	4	4.4	4.1	4.1	3.4	3.9	3.9	4.0	3.8	3.3	2.0	0.9	
5. Feijenoord.....D		10	10	10	10	10	10	10	5	7	10	10	0
W	2	4.8	4.3	4.5	5.2	5.7	6.6	6.8	5.6	5.8	[2]		
6. Heemaf.....D		—	0	0	0	0	0	2	5	5	6	5.25	0
[3]W	2	—	2.0	1.6	1.4	1.5	3.2	3.7	5.2	4.6	3.6	2.6	1.1
7. Van Gelder.....D		14	0	9	4	9	15	15	15	15	15	0	0
W	2	5.2	3.8	5.0	4.2	5.4	6.7	7.1	9.0	8.8	6.9	3.8	2.1
8. Scheepvaartunie.....D		14.75	7.2	8	7.3	8.9	10.5	11.4	12.6	12.7	11.3	6.9	0
W	2	8.1	4.9	5.2	5.8	6.2	7.1	7.9	8.3	8.8	6.8	4.2	1.8
9. Cult. Mij. der Vorstenl..D		45	12	10	12.5	17	15	7	14	12	10	0	0
W	2	9.0	7.2	8.1	8.3	7.1	6.9	7.3	7.5	6.4	5.2	3.1	1.4
10. Ned. Handelmij.....D		12	7	6	6	7	7.5	8	10	10	10	6.5	0
W	2	8.9	5.9	6.1	6.5	5.6	5.9	6.5	7.5	7.4	6.6	4.9	2.3
11. R'damsche Droogd....D		25	25	15	7	10	10	10	10	12	15	12	6
W	2	10.8	10.0	7.8	7.8	6.7	6.4	7.1	7.4	11.1	10.3	7.3	3.4
12. v. Nievelt, G. en Co....D		30	15	5	5	0	0	7	7	0	0	0	0
W	2	11.7	6.5	4.8	4.5	5.1	3.4	5.0	4.6	4.1	2.5	1.6	0.3
13. N. I. Suikerunie.....D		—	15	16	22.5	33.5	22.5	20	20	19.5	19.5	0	0
W	2	8.0	8.1	11.8	11.8	10.2	9.2	11.5	11.8	9.6	8.6	5.0	2.4

TABLE III, *continued*

14. Kolon. Bank	D		20	8	11	20	20	17	10	17	15	10	0	0
	W	3	6.6	5.6	6.4	7.9	7.1	7.8	11.0	9.9	9.2	6.0	3.6	2.4
15. Jurgens	D		15	0	0	0	0	[1]15	10	10	10	10	10	10
	W	2	7.4	3.5	3.1	2.8	4.2	6.6	7.2	11.3	17.7	11.7	6.5	4.0
16. Ned. Kunstzijdefabr.[5]	D		15	12	12	20	25	25	16	18	18	0	0	0
	W	2				12.2	17.6	13.0	13.8	21.3	14.6	4.2	2.7	1.1
17. Philips Gloeil.	D		31	11	11	11	16	16	16	21	21	21	6	4
	W	2	16.4	11.3	11.6	13.6	17.0	14.4	16.0	28.7	30.0	16.6	6.2	2.6
18. H.V.A.	D		60	25	30	35	40	35	30	30	30	30	15	5
	W	3	19.7	16.7	18.3	23.0	20.9	25.7	28.9	26.9	25.5	15.5	10.4	7.1
19. Kon. Petroleum	D		40	31	26.5	25	[6]23	23	23.5	24	24	24	17	5
	W	3	22.5	20.3	15.2	15.0	15.2	15.2	13.4	16.5	15.6	14.4	7.2	5.9
20. Wood trade (4 ent.)	D			2.7	6.4	6.8	9.4	9.3	9.3	9.2	7.9	3.7		
	W			3.4	3.3	4.2	4.3	4.8	5.2	4.8	4.7	(3.2)	(2.0)	(0.8)
21. Big banks (4 ent.)	D			7.3	6.0	6.4	4.4	5.3	6.6	6.9	7.4	7.5	6.5	
	W			5.3	4.6	5.0	4.2	5.2	5.3	5.4	5.7	5.2	4.8	(2.6)
22. Rubber plantations (3 enterprises)	D			0.7	4.7	9.7	8.2	25.7	23.9	17.2	11.8	10.7		
	W			2.3	4.4	5.3	6.0	13.1	11.8	8.4	8.3	(3.9)	(1.7)	(0.6)
23. All Dutch enterprises considered (69 ent.)	D			6.0	5.2	5.3	5.2	5.8	7.0	7.5	8.3	7.7		
	W			4.1	4.2	4.5	4.3	4.8	5.7	7.1	7.9			
24. All colonial enterpr. considered 23 ent.)[7]	D			9.1	8.2	11.3	15.5	16.5	15.2	15.7	15.4	13.5		
	W			6.6	6.9	8.0	8.3	9.5	10.6	10.3	9.7	(6.5)	(4.0)	(1.8)

[1] 1st quarter.
[2] Amalgamation with Wilton's Shipbuilding Yard.
[3] Up to 1926 W is given for the 4th quarter. Since, change of five year.
[4] Combined dividend for 3 years.
[5] Since 1929 Algemeene Kunstzijde Unie.
[6] Old stocks only.
[7] Without Petroleum Company. The "worth" of a stock is calculated by multiplying its price by the yield of 3 percent Dutch State Bonds.

TABLE IV.

STOCK PRICES (K), DIVIDENDS PAID (D), AND WORTH (W) FOR EIGHT DUTCH ENTERPRISES 1892–1919

Enterprise	Month		1892	1893	1894	1895	1896	1897	1898	1899	1900	1901	1902	1903	1904	1905
Amsterdamsche Bank	June	K	151	152	154	208	186	193	199	204	199	185	185	187	192	195
		D	7.5	7	7	7.5	9	8.5	8.5	9	9	8.5	8.5	9	9	8.5
		W	4.8	4.6	4.6	6.2	5.6	5.9	6.1	6.4	6.6	5.9	5.8	5.8	6.1	6.2
Cultuurmij. der Vorstenlanden...	June	K	80	86	88	60	58	51	*	*	53	51	34	41	71	102
* No data available		D	4.5	4	5	0	0	2.5	0	2.5	2	2	0	0	3.5	5.6
		W	2.6	2.6	2.6	1.8	1.7	1.5	*	*	1.7	1.6	1.1	1.3	2.2	3.3
Handelsvereeniging "Amsterdam"	July	K	102	90	100	100	100	103	98	111	114	111	103	107	113	154
		D	8.5	6	6.5	6	6.5	7	7	8	9	8	7	7	8	10
		W	3.2	2.8	3.0	3.0	3.0	3.1	3.0	3.5	3.8	3.5	3.2	3.3	3.5	4.9
Intern. Crediet- en H.V. "Rotterdam"	May	K	88	90	91	83	80	70	68	76	81	80	76	80	101	119
		D	4.4	4.4	4.4	4	3.2	3.2	3.2	4.2	3.2	3.2	3.2	3.2	5.2	4.4
		W	2.8	2.7	2.8	2.5	2.4	2.1	2.1	2.4	2.7	2.6	2.4	2.5	3.2	3.8
Javasche Bank	July	K	171	184	178	175	170	161	163	172	175	176	171	179	165	170
		D	3.3	7	9	8	8.1	6.6	6	7.5	7.7	9	8.5	9.3	9.3	9
		W	5.4	5.6	5.3	5.2	5.1	4.9	5.0	5.5	5.8	5.6	5.3	5.6	5.2	5.4
Koloniale Bank	July	K	80	83	85	61	54	45	38	46	47	45	35	29	34	72
		D	0	5	3.5	0	0	0	0	0	0	2	0	0	0	3
		W	2.5	2.5	2.5	1.8	1.6	1.4	1.2	1.5	1.6	1.5	1.1	0.9	1.1	2.3
Nederlandsche Bank	July	K	216	203	211	203	201	199	198	198	189	182	168	181	191	194
		D	8.6	7.5	8.9	6.9	7.6	9.0	9.5	8.7	10.8	10.4	9.4	9.7	10.9	7.8
		W	6.9	6.2	6.3	6.0	6.0	6.0	6.1	6.3	6.3	5.8	5.2	5.7	6.0	5.9
Ned.-Ind. Handelsbank	July	K	91	96	94	77	67	80	83	78	83	83	68	61	87	140
		D	4	6.3	5	0	2.3	4.5	3.3	4.7	5	5	2.9	0	5	9.1
		W	2.9	2.9	2.8	2.3	2.0	2.4	2.4	2.6	2.8	2.6	2.1	1.9	2.7	4.5
3 per cent Dutch State Bonds	May	K	94	98	99	101	100	98	98	96	91	93	95	96	94	94
	June	K	94	98	101	101	100	99	98	95	91	94	96	96	95	94
	July	K	95	98	101	101	101	99	97	94	90	94	97	96	96	94

TABLE IV, *continued*

Enterprise	Month		1906	1907	1908	1909	1910	1911	1912	1913	1914	1915	1916	1917	1918	1919
Amsterdamsche Bank	June	K	202	191	184	197	201	199	193	189	179	168	173	192	181	185
		D	10	11	8.5	9	10	10	9.5	11	10.5	9	11	13	12	12
		W	6.4	6.4	6.1	6.4	6.6	7.1	7.2	7.4	7.0	7.0	7.0	7.8	7.9	9.1
Cultuurmij. der Vorstenlanden	June	K	96	101	116	164	171	154	175	163	165	180	219	229	111	242
		D	5.8	6.75	7.5	8	7.25	8.5	10	10	10.5	10.5	15	20	0	15
		W	3.1	3.4	3.9	5.3	5.6	5.5	6.6	6.3	6.4	7.5	8.9	9.3	4.7	11.9
Handelsvereeniging "Amsterdam"	July	K	159	159	161	225	219	227	196	210	216	292	363	422	265	551
		D	10	10	10	10	10	10	10	10	15	25	25	30	25	25
		W	5.1	5.3	5.4	7.2	7.3	8.2	7.3	8.2	8.4	12.0	14.7	17.4	11.7	27.5
Intern. Crediet- en H.V. "Rotterdam"	May	K	135	144	148	175	208	199	206	208	198	172	208	222	207	257
		D	7.2	8	8.8	10.4	10.4	10.4	12	12.5	11.5	10	14	17	17	17
		W	4.3	4.8	4.9	5.7	6.9	7.0	7.6	8.0	7.6	7.2	8.7	9.1	9.1	12.2
Javasche Bank	July	K	185	185	190	209	225	231	243	240	249	245	251	271	259	305
		D	9.5	11	14	11.4	10.7	13.4	13.2	14	15.2	16	16	18	18	25
		W	5.9	6.2	6.3	6.7	7.5	8.4	9.1	9.4	9.7	10.0	10.2	11.2	11.4	15.2
Koloniale Bank	July	K	72	83	101	134	131	127	132	123	104	120	138	149	91	210
		D	3	5	6	6	7	7	7	8	6.5	6	8	12	5	7
		W	2.3	2.8	3.4	4.3	4.4	4.9	4.9	4.8	4.0	4.9	5.6	6.1	4.0	10.5
Nederlandsche Bank	July	K	192	195	208	206	201	210	212	225	223	227	225	216	175	189
		D	8.0	11.6	13.6	8.8	8.3	11.7	10.2	12.2	13.4	13.1	13.4	11.2	13.7	20.7
		W	6.1	6.5	6.9	6.6	6.7	7.6	8.0	8.8	8.7	9.3	9.1	8.9	7.7	9.4
Ned.-Ind. Handelsbank	July	K	129	130	129	173	280	265	236	215	207	181	215	213	155	285
		D	10.2	8.7	8	10	12	23	18.5	15	12	10	14	23	12	17
		W	4.1	4.3	4.3	5.5	9.3	9.6	8.9	8.4	8.1	7.4	8.7	8.8	6.8	14.2
3 per cent Dutch State Bonds	May	K	94	89	90	92	91	85	81	78	78	72	72	73	69	63
	June	K	94	89	90	93	91	84	80	77	77	72	74	74	69	61
	July	K	94	90	90	94	90	83	80	77	77	73	74	73	68	60

TABLE V
STOCK PRICES AND DIVIDENDS PAID, 1921–1932, FOR THREE JAVA RUBBER PLANTATIONS

Enterprise	Year	Stock prices												Dividends
		Jan.	Feb.	March	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	
1. Amsterdam Rubbercultuur Maatschappij	1921	141	134	116	95	105	86	96	97	91	94	104	106	0
	1922	101	93	85	87	78	61	52	66	75	98	110	116	0
	1923	138	149	149	165	155	149	145	152	155	144	147	145	8
	1924	153	163	148	143	132	117	133	147	151	159	158	162	12
	1925	165	167	189	211	225	229	284	285	318	368	397	386	15
	1926	356	356	344	331	332	313	323	318	334	333	324	316	25
	1927	331	332	351	333	342	307	288	304	298	293	311	323	25
	1928	319	295	264	233	239	230	230	234	228	231	236	232	18
	1929	262	282	275	260	264	256	261	266	266	254	200	167	15
	1930	160	175	148	152	141	121	123	105	89	99	110	104	15
	1931	103	109	111	88	83	73	86	68	50	54	54	44	0
	1932	47	46	41	30.6	30	29.9	42	62	68	62			0
2. Serbadjadi-Sumatra-Rubber-cultuurmij	1921	230	229	197	158	175	139	150	176	169	177	197	227	0
	1922	221	213	208	211	202	193	170	184	197	232	240	267	17
	1923	280	285	293	323	288	282	284	297	299	286	290	262	20
	1924	227	234	211	202	188	177	175	194	193	194	193	203	18
	1925	214	214	220	223	240	238	278	267	300	363	426	410	17
	1926	382	377	365	348	343	313	325	322	339	337	328	321	30
	1927	337	335	352	344	339	306	291	310	299	288	306	329	25
	1928	328	303	256	205	204	201	178	185	172	175	188	181	20
	1929	214	238	235	203	200	187	203	205	206	197	143	120	8
	1930	118	127	113	120	105	85	84	70	58	59	64	62	0
	1931	62	68	71	48	36	37	48	34	20.4	25.5	23.8	19.1	0
	1932	19.3	19.5	15.0	7.1	7.8	7.0	12.5	24.9	30	22.5			0
3. Deli-Batavia Rubbermij	1921	111	106	90	73	85	67	71	77	66	65	71	74	0
	1922	71	64	58	56	47	37	29	37	40	59	65	72	0
	1923	82	81	82	90	84	75	76	79	78	73	76	78	0
	1924	82	82	83	79	72	68	75	83	87	88	87	91	0
	1925	108	110	118	122	140	147	180	179	211	241	281	273	0
	1926	257	255	241	235	241	228	248	244	267	269	259	250	15
	1927	265	269	278	271	281	254	234	248	242	243	252	254	20
	1928	252	235	199	165	168	161	154	164	152	155	163	160	10
	1929	179	196	191	173	173	164	172	175	174	168	127	103	10
	1930	95	110	107	107	93	77	78	62	46	50	60	54	0
	1931	54	60	63	44	41	29	42	31	19	21.2	21.5	18.4	0
	1932	19.7	19.3	14.8	7.6	8.8	8.5	15.5	27.5	33	27.3			0

TABLE VI

RUBBER PRICE AND YIELD ON DUTCH STATE BONDS, 1921–1932

	Year	Jan.	Feb.	March	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
Rubber price in (Dutch) cents per ½ kg	1921	68	71			48	40	50	47	45	50	56	60
	1922	52	45	43	45	41	42	40	39	39	53	67	74
	1923	89	96	95	89	77	76	76	82	82	76	65	65
	1924	74	73	69	68	58	56	59	68	76	87	97	101
	1925	102	99	112	118	150	197	253	236	224	243	276	294
	1926	224	175	157	145	126	116	114	108	113	116	110	102
	1927	108	106	111	113	112	102	94	97	92	93	102	109
	1928	106	89	70	58	50	51	52	52	48	48	48	49
	1929	54	67	66	58	59	59	61	58	55	52	46	43
	1930	41	43	42	40	38	35	30	26	23	22	24	24
	1931	22	21	21	17	17	17	17	14	13	13	13	12
	1932	12	11	12	11	10	9	9	11	12	11		
Yield on 3 per cent Dutch State Bonds.	1921	5.55	5.36	5.15	5.13	4.98	4.83	4.84	4.76	4.86	4.85	4.95	5.42
	1922	5.28	5.16	5.04	4.88	4.80	4.79	5.22	4.88	4.69	4.83	4.83	4.83
	1923	4.83	4.79	4.68	4.57	4.49	4.47	4.52	4.52	4.66	4.71	4.69	4.92
	1924	4.70	4.79	4.81	4.90	4.89	4.74	4.55	4.50	4.47	4.53	4.53	4.49
	1925	4.39	4.38	4.27	4.32	4.25	4.08	4.09	4.04	4.07	4.03	4.07	4.12
	1926	4.09	4.10	4.08	4.07	4.06	4.03	3.98	3.97	3.97	3.97	3.96	4.00
	1927	3.97	4.04	4.01	4.05	4.01	4.05	3.97	3.89	3.99	3.97	4.08	4.04
	1928	3.99	3.94	3.87	3.88	3.91	3.87	3.89	3.85	3.84	3.82	3.83	3.87
	1929	3.86	3.83	3.84	3.84	3.96	3.92	3.96	3.94	3.96	3.97	3.99	3.99
	1930	3.92	3.86	3.85	3.87	3.85	3.78	3.79	3.80	3.69	3.73	3.69	3.67
	1931	3.64	3.70	3.69	3.81	3.82	3.70	3.75	3.73	3.81	4.24	4.15	4.59
	1932	4.30	4.14	4.05	3.99	4.13	3.88	3.80	3.72	3.67			

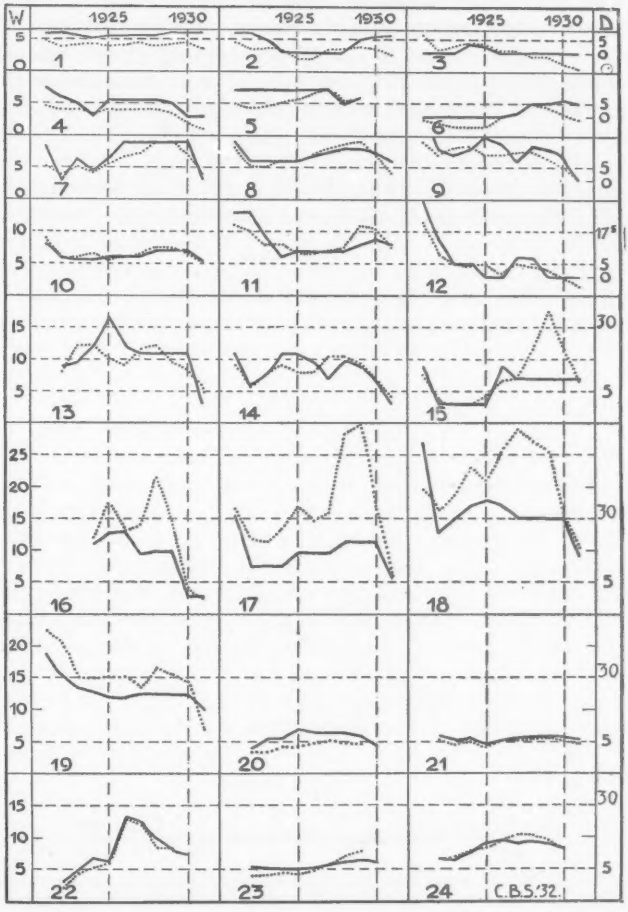


FIGURE 2. ..... Dividend (D) paid, and —— Worth (W) in the quarter of dividend payment, for 19 Dutch enterprises and 5 groups of enterprises, 1921–1932.

The scale of this chart has been chosen in such a way that the lines cover each other when the relation

$$W = 3 + 0.4D.$$

is satisfied. This relation represents the relation found for the totality of the material, by rough estimate.

The explanation of the numbers and the figures for D and W are given in Table I.

Figs. 2 and 3 and Table III relate to a number of Dutch stocks, selected out of several branches of industry, for the period 1921–1931. For each stock considered, D and W have been calculated; figures relate to the month in the middle of the quarter in which dividend was paid.[4] The calculations were made for 19 leading companies separately and, in addition, for 5 groups of enterprises, as indicated in Table III. The results are represented graphically in two ways. Fig. 2 compares, for each case, D and W. The scales for D and W are taken different, such that the curves for D and W are identical when the relation

$$W = 3 + 0.4D \tag{1}$$

is satisfied. This was, roughly estimated, the relation found for the majority of cases to hold fairly well. So Fig. 2 shows the degree of ac-

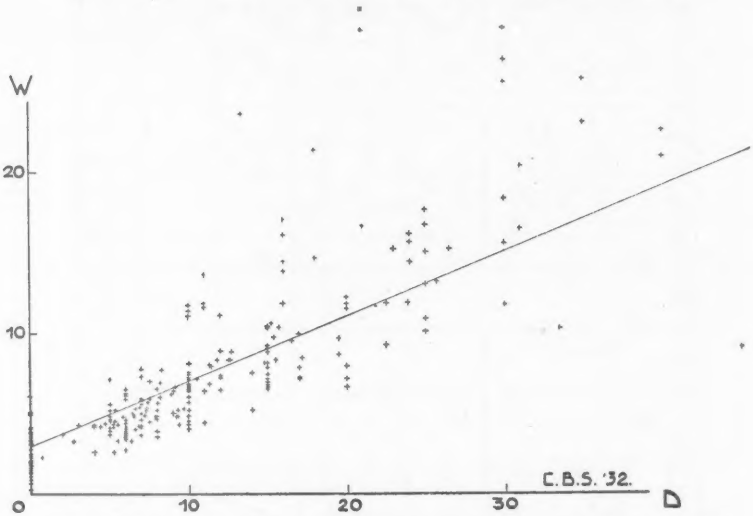


FIGURE 3.—D and W for the enterprises and groups considered (1921–1932). Straight line:

$$W = 3 + 0.4D$$

cordance for the individual stocks. It is interesting that the deviations from relation (1) are largest for the really speculative stocks Nos. 15–19.

In Fig. 3, corresponding values of D and W, as given in Table III

[4] This method of calculation was chosen for reasons of simplification. A number of tests indicated that no great differences occur when other possible methods are followed.

and Fig. 2, have been plotted against each other. The same has been done for 8 stocks (banking and colonial agriculture only) for which data were easily to be found, in the period 1892–1919. The result is not very different. Owing to rough graphical calculation the relation between D and W for these stocks is about (cf. Fig. 4):

$$W = 1.5 + 0.47D \qquad (2)$$

The significance of the result, in connection with the fairly good correlation between the two members of the equations given, may be formulated as follows: The chief determinant factor to W, i.e., to dividend expectancies, is the last dividend paid.[5] The movements of D, however, are reflected only about half as intensively. This shows that reactions are always expected in about such a way that the total picture of the future corresponds to a dividend of about "half as abnormal" as the last dividend paid. As "normal" dividend D_0 occurs, then, the dividend for which $W = D$, i.e., 5 per cent for the decade after the war, and about 3 per cent for pre-war years.

The most interesting feature in our result is illustrated by Fig. 7. To fix the ideas, τ is indicated by the distance shown at the bottom. D may indicate the real course of dividend through a business cycle. As we just have seen that for the period τ there exists an average dividend expectance equivalent to a "normal" dividend D_0 plus about half the excess of D over D_0, we can, also, to fix the ideas, suppose that the expected course of D at each moment is indicated by the dotted lines. The essential feature of those lines is that something like the surface between each such line and the D_0-axis should be proportional to the height of D over D_0. Although a conclusion might be premature, this diagram suggests the absence of any "forecasting quality" of stock prices.

III

SHORTER MOVEMENTS OF STOCK PRICES

The above-considered relation between D and W only determines the annual movement of W, and therefore of stock prices. For shorter movements, other determining factors exist, which is illustrated by Figs. 5 and 6 for rubber plantation stocks. Fig. 5 again shows the agreement between D and (twelve month averages of) W; Fig. 6 illustrates the correlation between the "shorter movements" of W (viz. deviations from 12 months' averages) and those of rubber prices P. (Numbers 1, 2 and 3 relate to three different enterprises.)

[5] Cf. also *Wochenbericht des Instituts für Konjunkturforschung*, 15 Nov. 1932, p. 133, giving similar results of an investigation by Dr. O. Donner.

As to the correlation shown in Fig. 6, the following remarks may precede our further considerations: The best agreement exists between W_i and percentage values of P (i.e., P in per cent of 12 months' moving

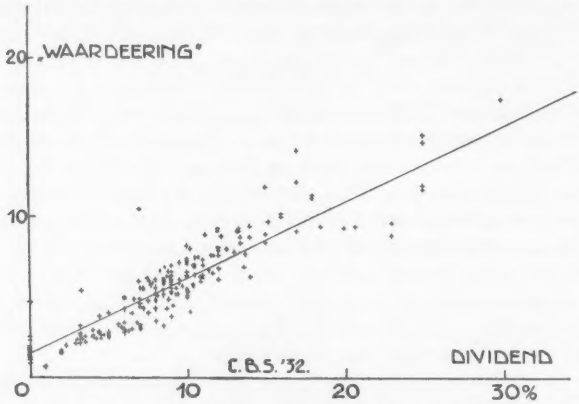


FIGURE 4.—D and W, for the enterprises considered, (1892–1919). Straight line:

$$W = 1.5 + 0.47D.$$

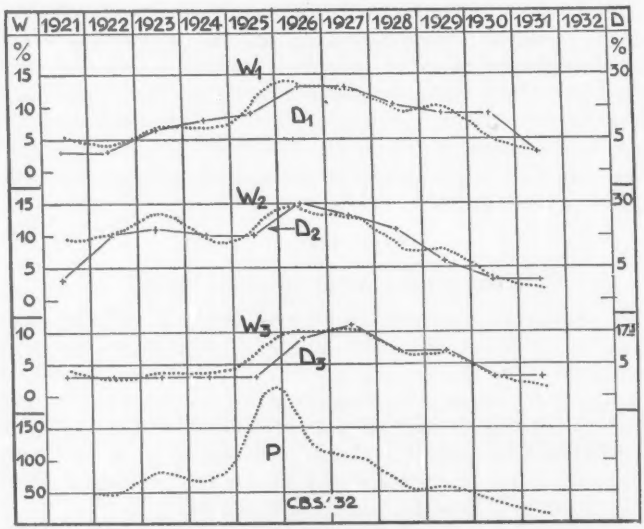


FIGURE 5.—W and D for three Java rubber plantations, 1921–1932. W's: 12 months' moving averages. The indexes relate to the three enterprises mentioned in Table III. Scale similar to that of Fig. 1.

averages, heavy line); logically one should expect W to correlate with the absolute price movements (as equal absolute price increments cause equal dividend increments), indicated by the dotted line. This correlation is, however, far poorer; and it remains still a little poorer when a multiple correlation between W on one side and P and K on

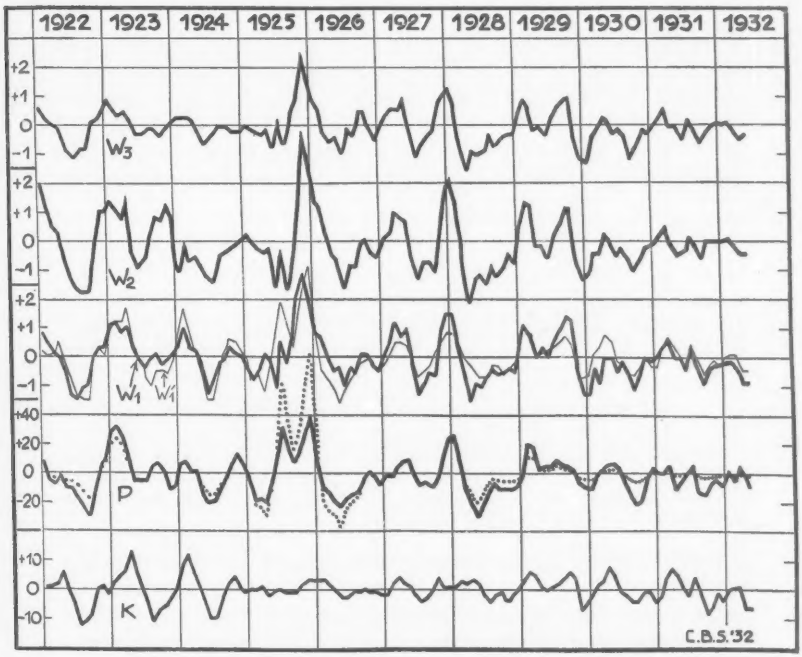


FIGURE 6.—Short fluctuations of worth of rubber stocks, W, as compared with short fluctuations in rubber price, P, and in general index of stock prices, K (1921-'25=100). The indexes relate to the enterprises mentioned in Table III. All series: deviation from 12 months' moving averages. Heavy line P: percentage deviations; dotted line P: absolute deviations, in cts per ½ *kg*. W_1', calculated from formula:

$$W_1' = 0.035P + 0.097K,$$

in which P represents absolute price deviations, giving maximum correlation with W_1.

the other side is considered $-K$ indicating general stock price index (deviations from 12 months' averages). This is shown by the line W_1', giving the "best combination" of K and (dotted) P. As far as I can see a simple explanation of this fact is not easy.

We may consider the matter also from another angle. Supposing the correlation between the absolute price deviations and W is satisfactory, what is then to be deduced, with the help of our notions "horizon" and "expectancy," from the regression coefficient?

From that coefficient we deduce that, for the short movements, 1 cent of price difference (per ½ kg) corresponds to a difference in 0.035 per cent of W_1, i.e., of dividend expectance during the period of τ. Now from other sources that are known to every investor, viz. the profit and loss account,[6] I deduce that a price movement of 1 cent causes a dividend movement (during the period 1921–1931, with large increases and large decreases) of 0.3 per cent. If the investor expected the deviation of price from normal to apply for his whole τ, then he had to expect, for this τ, a dividend of 0.3 per cent multiplied by the

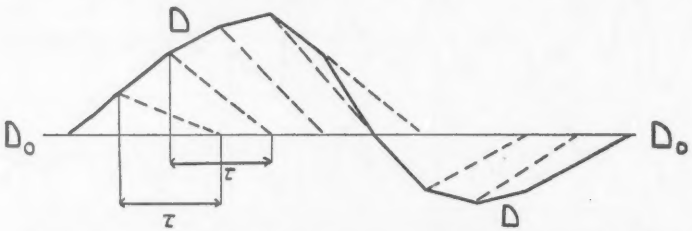


FIGURE 7.—Character of Dividend expectances. D: actual course of dividends. Dotted lines: expectances at each moment; τ: length of "horizon," chosen arbitrarily. The essence of the scheme is that at each moment expectances gravitate to D_0, a sort of "normal" dividend.

price deviation in cents. As he only seems to expect 0.035 per cent, our conclusion is that he expects price to move back again within the period τ, such that "on the average" only a deviation of about 0.1 of the deviation observed will exist.

So, although it was not possible in the second and third problem presented to evaluate the horizon, it seems, nevertheless, possible to learn something about the expectancy.

University of Amsterdam, Holland

[6] See e.g. Van Oss' *Effectenboek* 1932 I, 1084–1088.

LA DEMANDE DANS SES RAPPORTS AVEC LA RÉPARTITION DES REVENUS

Par René Roy

Communication présentée à la réunion de la Société d'Économétrie à Lausanne, septembre 1931

Ainsi qu'il advient fréquemment, ce sont des considérations d'ordre professionnel et par conséquent d'ordre pratique, qui nous ont conduit à effectuer quelques recherches sur les lois de la Demande.

Pendant la période de trouble monétaire qui sévit en France jusqu'en 1927, il a été nécessaire de procéder à de nombreux ajustements de tarifs dans l'exploitation des services publics; il nous a été ainsi possible d'observer sur un assez grand nombre de réseaux de chemins de fer et de tramways urbains, la manière dont le trafic était affecté par les augmentations successives de tarifs.

Par la suite, nous en vînmes à des recherches présentant un caractère beaucoup plus théorique, et ces études ont fait l'objet d'un mémoire présenté au congrès international des mathématiciens de Bologne (Septembre 1928), et publié par la Revue Internationale de Statistique *Metron* dans son numéro du 28 Février 1930 sous le titre "La Demande dans ses rapports avec la répartition des revenus."

LE CARACTERE ABSTRAIT DE LA LOI DE LA DEMANDE

Notre conception de la loi de la Demande est celle qu'envisageait Cournot, lorsqu'il considérait les variations du "débit" d'un article en fonction de son prix.

Pour cet auteur, la loi de la Demande implique la permanence de tous les autres éléments de l'équilibre économique, tels que le prix et le débit de tous les autres articles, l'effectif de la population, le montant des revenus individuels et leur répartition parmi les membres de la collectivité, les goûts et les habitudes, l'influence de la mode, etc. Nous savons qu'en réalité, semblable permanence n'existe pas, puisque les éléments dont il s'agit varient constamment au cours du temps et présentent des liens d'interdépendance, tels que la modification d'un certain nombre d'éléments entraîne inévitablement celle de tous les autres.

Cependant, lorsqu'il existe sur un marché un grand nombre d'échangistes et une semblable diversité de marchandises, on se trouve fondé à négliger l'influence des variations de prix ou de débit d'un article sur le prix et le débit des autres articles, sous réserve bien entendu, de ne pas tenir compte des phénomènes de substitution; d'autre part, on conçoit aussi la possibilité de considérer comme invariables d'autres paramètres tels que l'effectif de la population, les revenus individuels

et leur répartition, etc., en se réservant d'examiner ultérieurement l'action de telles variations sur les lois de la Demande.

Ainsi dépouillée de toutes les influences perturbatrices, la loi de la Demande exprime la relation qui existe à un moment donné entre la Demande collective d'un article et son prix unitaire; d'après ce qui précède, cette loi se présente bien avec un caractère abstrait, et d'autre part, nous sommes contraints, pour le moment tout au moins, de lui assigner un caractère empirique, puisqu'a priori, nous ne pouvons rattacher la loi de la Demande à une théorie rationnelle.

INFLUENCE DE LA DISTRIBUTION DES REVENUS INDIVIDUELS

Pour un individu pris en particulier, le montant des sommes qu'il consacre à l'acquisition d'une marchandise déterminée, dépend évidemment de son revenu; si l'on considère par conséquent la demande totale d'un article, la quantité demandée à un certain prix dépendra du nombre d'individus possédant un revenu suffisant pour leur permettre d'acquérir cet article; en d'autres termes, la loi de la Demande est liée à la loi de distribution des revenus individuels au sein de la collectivité étudiée.

Or, la loi de distribution des revenus individuels a déjà donné lieu à d'importantes recherches qui ont permis d'aboutir à des conclusions assez précises; nous avons en particulier considéré la formule simple de Pareto, qui définit le nombre N d'individus possédant un revenu supérieur à une valeur x déterminée, conformément à l'équation:

$$N = \frac{A}{x^\alpha},$$

A désignant un coefficient constant et α un exposant supérieur à l'unité variable selon les pays et les époques, mais dont la valeur moyenne est voisine de $\frac{3}{2}$.

Sans méconnaître l'imperfection de cette formule simple, nous l'avons utilisée, parce qu'elle traduit la tendance selon laquelle se groupent les revenus individuels; son utilisation permet aisément de calculer le nombre des personnes ayant un revenu compris entre deux limites déterminées, ainsi que la valeur totale des revenus détenus par ces personnes, soit:

$$\begin{cases} N_1^2 = N(x_2) - N(x_1) = \dfrac{A}{x_2{}^\alpha} - \dfrac{A}{x_1{}^\alpha} \\ R_1^2 = \displaystyle\int_{x_1}^{x_2} x\,dN = -\alpha A \int_{x_1}^{x_2} \frac{x\,dx}{x^{\alpha+1}} = -\alpha A \int_{x_1}^{x_2} \frac{dx}{x^\alpha} \\ \qquad = \dfrac{\alpha A}{\alpha - 1}\left[\dfrac{1}{x_2{}^{\alpha-1}} - \dfrac{1}{x_1{}^{\alpha-1}}\right]. \end{cases}$$

Il va sans dire que toute autre loi de distribution des revenus, même si elle n'était pas définie analytiquement, mais simplement représentée par un diagramme, serait susceptible d'être utilisée avec fruit, au prix d'une complexité plus grande des calculs.

L'AFFECTATION DES REVENUS INDIVIDUELS

Pour déduire la formule générale de la loi de la Demande, de celle qui représente la distribution des revenus individuels, nous avons été obligés de faire certaines hypothèses sur la manière dont les individus se comportent dans l'affectation de leurs ressources à l'acquisition des diverses marchandises. Nous devons insister sur ces hypothèses qui, comme toutes les hypothèses, sont sujettes à discussion, et pourront être ultérieurement révisées, si les conclusions qui en découlent, sont démenties par les faits.

En premier lieu, nous constatons que les individus possédant un même revenu n'acquièrent pas nécessairement les mêmes marchandises; cependant, il est des besoins qui sont plus impérieux que d'autres et cela pour n'importe quel individu.

On voit ainsi que, dans l'affectation d'un revenu déterminé, tout individu sera guidé par deux sentiments: le degré d'urgence de ses divers besoins d'une part, ses goûts d'autre part.

On conçoit ainsi la possibilité d'établir, parmi l'ensemble des marchandises de consommation, une classification en groupes répondant à la hiérarchie des besoins et telle que tout individu chercherait à se procurer les objets d'un groupe, de préférence à ceux du groupe supérieur, se réservant d'aborder ce groupe supérieur, si ses ressources le lui permettent; à l'intérieur de chaque groupe, les individus effectueraient leurs choix d'après leurs goûts personnels. Il paraît difficile de marquer en pratique les frontières de ces groupes, mais il est certainement possible d'en concevoir l'existence.

En second lieu, nous admettons que les groupes sont constitués de telle manière qu'un individu ne passe au groupe supérieur que s'il dispose d'un revenu qui lui permette de satisfaire intégralement les besoins correspondants aux groupes inférieurs; autrement dit, chaque individu affecte successivement les diverses fractions de son revenu à la satiété des besoins les plus impérieux, pour passer ensuite aux besoins d'ordre plus élevé.

Enfin, nous supposons en dernier lieu que le revenu consacré à l'acquisition des biens d'un groupe, pour acquérir la satiété des besoins correspondants, ne dépend pas de la valeur du revenu individuel; c'est là une hypothèse fort contestable et probablement inexacte, mais elle peut cependant se justifier par les considérations suivantes:

Si l'on considère les sommes affectées par les personnes disposant

d'un revenu assez élevé, à la satisfaction des besoins d'ordre inférieur, ces sommes ne représentent qu'une faible fraction du revenu de ces personnes, et d'autre part, le nombre de ces personnes est peu élevé, dès qu'il s'agit d'un revenu assez important. On ne commet donc pas une erreur très lourde en admettant la constance des sommes consacrées à la satiété des besoins afférents à un groupe déterminé; d'ailleurs, cette hypothèse pourrait éventuellement faire place à une autre, sans que la marche générale de notre raisonnement en soit affectée.

Le caractère éminemment discutable de nos hypothèses marque bien notre intention de n'établir qu'une théorie provisoire; il sera toujours possible d'en améliorer les résultats en faisant appel à des hypothèses moins simples, si ce choix nous est imposé par la suite, sous l'empire d'un plus grand souci de précision. Nous ne prétendons pas édifier une théorie définitive et nous sommes au contraire persuadés que notre ébauche sera par la suite considérablement améliorée.

CARACTERES GÉNÉRAUX DES COURBES DE DEMANDE

Du moment qu'il est fait appel, non pas à des articles uniques, mais à des groupes d'articles, il est nécessaire de caractériser chacun de ces groupes par des index de prix et de quantité, P et Q, tels que:

$$PQ = k \sum qp$$

Σpq désignant le total des payements consacrés par l'ensemble des consommateurs à l'acquisition des objets du groupe.

La loi de la Demande sera définie dans ces conditions par une relation fonctionnelle, entre les index P et Q.

Pour déterminer cette relation, nous sommes conduits à envisager parmi les consommateurs deux ensembles distincts: (a) Ceux dont le revenu est assez élevé pour permettre la satiété des besoins satisfaits par le groupe d'articles considéré et par les groupes d'ordre inférieur; (b) Ceux dont le revenu ne permet pas de satisfaire intégralement les besoins correspondant au groupe envisagé.

En se fixant la quantité correspondant à la satiété du besoin considéré, il est possible d'obtenir, pour chaque valeur de P le total des sommes consacrées à l'acquisition des objets inclus dans le groupe, et par conséquent, de définir la relation qui existe entre P et Q, autrement dit, l'équation de la courbe de la Demande.

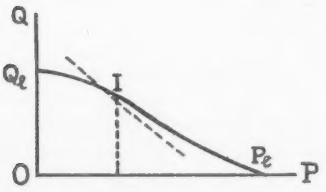
L'équation ainsi obtenue diffère, suivant que l'on considère le groupe des objets de première nécessité, ou au contraire, un groupe répondant à des besoins moins urgents, et supposant par conséquent la possession d'un revenu au moins égal à celui qui permet d'obtenir la satiété des besoins d'ordre inférieur.

Pour les objets de première nécessité, si l'on admet l'existence dans

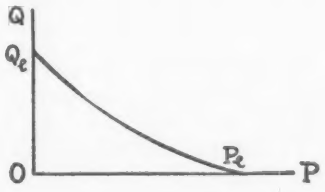
la loi de distribution des revenus d'un minimum auquel tous les revenus individuels sont supérieurs, il existe un prix pour lequel tous les individus ont la possibilité de se procurer la satiété des besoins correspondant au groupe; la demande atteint son maximum pour ce prix et demeure constante pour les prix inférieurs. Pour les marchandises autres que les objets de première nécessité, il n'existe pas au contraire de maximum et la consommation ne cesse de croître jusqu'à une valeur finie, lorsque le prix diminue.

Comme d'autre part, la concavité de la courbe de la demande est orientée vers la partie positive de l'axe des quantités, pour un prix suffisamment élevé, puisque la courbe est asymptote à l'axe des prix, on constate ainsi que, pour ces objets de première nécessité, la courbe de la demande présente un point d'inflexion, et ce résultat est conforme à une observation faite par Edgeworth.

Si l'on veut déduire de ces résultats la forme générale des courbes de demande correspondant, non plus à un groupe, mais à un article unique, on doit tenir compte de l'existence d'un prix-limite, au-delà duquel ne subsiste plus aucune demande, et l'on doit également faire état des phénomènes de substitution, qui tendent à introduire des discontinuités dans la courbe théorique; on aboutit de cette manière aux diagrammes ci-après pour lesquels est toujours maintenue la distinction entre les objets de première nécessité et les produits autres que les objets de première nécessité.



Demande relative aux objets de première nécessité.



Demande relative aux objets autres que ceux de première nécessité.

Nous observons en particulier que pour les objets de première nécessité, il est fort possible que dans un état suffisamment avancé du bien-être matériel, le prix courant se trouve inférieur à l'abcisse du point d'inflexion; de la sorte, la concavité de la courbe au voisinage du prix courant serait orientée vers la partie négative de l'axe des quantités. Toutefois, dans la plupart des cas, il semble bien que la concavité soit orientée dans le sens le plus fréquemment adopté.

EXPRESSION DIFFÉRENTIELLE DE LA LOI DE LA DEMANDE

Si l'on considère l'équation différentielle de la loi de la Demande, on peut l'exprimer par la relation:

$$\frac{dQ}{Q} = -\lambda\frac{dP}{P}.$$

Dans cette relation, λ désigne ce que Marshall a qualifié "coefficient d'élasticité."

L'expression de ce coefficient est la suivante:

$$\lambda = \frac{\alpha x^{\alpha-1} - \alpha}{\alpha x^{\alpha-1} - 1}.$$

Dans cette expression x désigne le rapport du prix P au prix qui correspond à la consommation susceptible de procurer la satiété; ce rapport varie donc entre $+1$ et $+\infty$; l'exposant α n'est autre que celui de la formule de Pareto, voisin de 3/2 et par conséquent supérieur à l'unité.

Lorsque x varie de $+1$ à $+\infty$, λ varie de 0 à $+1$, et ses variations sont excessivement lentes, en raison même de sa forme algébrique; pour les besoins de la pratique, on peut donc admettre en première approximation que λ reste égal à une valeur constante λ_0, à condition de n'envisager pour les variations de prix, qu'un intervalle suffisamment restreint; on aboutit de cette façon à une relation simple, qui peut être directement utilisée pour les applications concrètes, soit:

$$\frac{dQ}{Q} = -\lambda_0\frac{dP}{P}.$$

Le coefficient λ_0 est un nombre pur, indépendant des unités choisies; il permet de mesurer les facultés de variation de la consommation en fonction des variations de prix, c'est-à-dire l'élasticité de la demande.

La demande est d'autant plus élastique que λ_0 est plus élevé, et l'on peut ainsi classer les divers produits selon les valeurs du coefficient d'élasticité, de telle manière que les besoins correspondant à chacun des articles soient d'autant plus pressants que le coefficient d'élasticité est moins élevé.

Il est d'autre part essentiel de noter que, d'après la formule ci-dessus définie, le coefficient d'élasticité reste toujours inférieur à l'unité; il en résulte d'importantes conséquences:

Si l'on considère en effet la recette R procurée par la vente de Q unités au prix P, on obtient la relation:

$$R = PQ$$

$$\frac{dR}{R} = \frac{dP}{P} + \frac{dQ}{Q} = \frac{dP}{P}(1 - \lambda).$$

Il suit de là que la recette est fonction croissante du prix, et par conséquent fonction décroissante de la quantité vendue.

Toutefois, ce résultat provient de l'expression de λ et, dans la pratique, on doit tenir compte des divergences observables par rapport à la courbe théorique, notamment en raison de l'existence des phénomènes de substitution.

Les observations que nous avons faites tendent cependant à démontrer qu'en pratique, le coefficient d'élasticité reste bien inférieur à l'unité.

DÉTERMINATION PRATIQUE DU COEFFICIENT D'ÉLASTICITÉ

Les observations que nous avons faites ont porté presque toutes sur les variations de trafic des services publics, provoquées par des modifications de tarifs.

Cette méthode revient somme toute à pratiquer une véritable expérimentation, dans laquelle les différents facteurs de l'équilibre économique peuvent être regardés comme constants, tandis que la variation de tarif jouant le rôle de cause détermine une variation de trafic, jouant le rôle d'effet.

Nous résumons comme il suit les résultats acquis.

1° *Consommation du gaz d'éclairage à Paris.*—L'examen des conséquences de la réduction du prix du mètre cube de gaz de 0f. 30 à 0f. 20, conduit à un coefficient d'élasticité de 0.50 environ.

2° *Influence de la variation des tarifs postaux.*—Les résultats observés à l'occasion des réductions de tarif effectuées en France en 1878 (0f. 15 au lieu de 0f. 25) et en 1906 (0f. 10 au lieu de 0f. 15) ont permis d'aboutir à un coefficient de 0.75.

3° *La demande en matière de transports de voyageurs.*—Pour les transports en commun, nous avons observé que le coefficient d'élasticité varie entre 0.25 pour l'agglomération parisienne et 0.50 pour les villes de moindre importance (0.32 environ pour les tramways de Marseille) en suivant une progression proportionnée au chiffre de la population. Pour les réseaux d'intérêt général, les relèvements opérés en 1926 ont conduit à un coefficient d'élasticité de l'ordre de 0.40.

4° *Observations relatives au monopole des tabacs en France.*—En utilisant les données fournies par M. Bérend, ingénieur des manufactures de l'État, données qui ont permis d'éliminer l'influence du stockage, et des variations saisonnières, nous avons obtenu les coefficients d'élasticité suivants:

(a) Relèvement d'Avril 1923 $\lambda = 0.66$
(b) Relèvement de Mars 1925 $\lambda = 0.70$
(c) Relèvement d'Août 1926 $\lambda = 0.79$

D'autre part, les éléments inclus dans la loi de Gregory King, formulée à propos du blé, permettent de constater que le coefficient d'élasticité varie entre 0.28 et 0.37, autour d'une valeur moyenne de 0.31, tandis que Q varie du simple au double et P du simple au quintuple.

A notre méthode d'expérimentation, s'oppose la méthode statistique du Professeur Schultz, appliquée à la consommation du sucre aux États-Unis, entre 1890 et 1914.

Par un ajustement statistique, fondé sur la méthode des moindres carrés on obtient, entre les variations relatives de prix u et les variations relatives de quantité v, la relation linéaire:

$$v = 1.58 - 0.53u.$$

Le terme constant du second membre représente la tendance séculaire correspondant à une loi exponentielle, tandis que le coefficient angulaire 0.53 n'est autre que le coefficient d'élasticité, traduisant pour la demande du sucre une élasticité intermédiaire entre celle du blé et celle du tabac.

La méthode d'ajustement linéaire utilisée par M. Schultz dépouille ainsi son caractère arbitraire, et les coefficients utilisés acquièrent tout leur sens, à la lumière de la théorie précédemment développée sur les lois de la Demande.

Il n'est pas douteux que la méthode statistique soit appelée à rendre en ce domaine d'éminents services, mais il est nécessaire que de telles recherches soient appuyées sur des conceptions théoriques précises, sans lesquelles la méthode statistique serait impuissante.

CONCLUSIONS

(1) D'après ce qui précède, le coefficient d'élasticité demeure inférieur à l'unité et il est d'autant moins élevé que l'article auquel il correspond, satisfait à un besoin plus urgent.

La considération du coefficient d'élasticité permet ainsi d'établir une classification entre les divers services ou produits directement consommables, les objets de première nécessité se trouvant caractérisés par une demande peu élastique et les produits de moindre utilité répondant eux-mêmes à un coefficient d'élasticité sensiblement plus élevé.

(2) Il semble d'autre part résulter des observations faites qu'étant donné un objet de consommation, dont la quantité produite annuellement peut subir d'assez fortes variations, les écarts de prix résultant de ces variations soient tels que la valeur totale de la marchandise envisagée varie en sens inverse de la quantité produite.

S'il était possible d'établir la généralité de la proposition, celle-ci permettrait d'expliquer certaines tendances relevant du "malthusia-

nisme économique" et dont le but est de réduire la production, pour accroître la valeur du produit.

Au cas où la loi serait étendue à la demande de travail, elle justifierait, vis-à-vis des salariés, les mesures tendant à restreindre les disponibilités en heures de travail, puisque le montant total des salaires serait d'autant plus élevé que le nombre d'heures de travail serait plus réduit.

(3) Les exemples empruntés à divers monopoles conduisent à penser que les tarifs appliqués restent toujours inférieurs à ceux qui procureraient la recette brute maxima.

De plus, lesdits exemples semblent prouver que tout abaissement de tarif se traduit par une diminution de la recette brute, malgré l'accroissement de la consommation provoqué par l'abaissement de tarif.

École Nationale des Ponts et Chaussées

A COMPARISON OF ELASTICITIES OF DEMAND OBTAINED BY DIFFERENT METHODS[1]

By Henry Schultz

A summary of this paper was read before the joint meeting of the Econometric Society with the American Statistical Association, Washington, D. C., December 29, 1931.

I. INTRODUCTION

I believe that inductions with regard to the elasticity of demand, and deductions based on them, have a great part to play in economic science.—Alfred Marshall, "On the Graphic Method in Statistics," *Jubilee Volume of the Roy. Statist. Soc.* (1885), p. 260.

Although these dicta of the great teacher were written nearly fifty years ago, and although the great Cournot, Marshall's main source of inspiration, had expressed a similar conviction over half a century before,[2] it was not until 1914 that the first definitive attack on the problem of deriving the elasticity of demand from statistics was made. In that year Professor Henry L. Moore published his *Economic Cycles: Their Law and Cause*, in which he obtained equations expressing the relations between the demands for, and the prices of, corn, hay, oats, and potatoes; determined the precision of these equations as formulas for estimating prices; and measured the elasticity of demand for each crop. True, we now know that Moore's attempt to derive the numerical values of the elasticities of demand of different commodities was anticipated by Professor Pigou[3] in 1910, and by Professor Tschayanow[4] in 1912, who adopted an entirely different method of approach; and that in the same year in which Moore's book appeared, Professor R. A.

[1] This study was made possible through the generosity of the Social Science Research Committee of the University of Chicago. Miss Frances Gibson, Miss Edith Mohn, and Miss Janet Murray, collaborated during the various stages of this investigation, and did most of the statistical work. Mr. Francis McIntyre and Mr. Sidney Sufrin gave me part-time assistance. The charts are the work of Messrs. Lester Kellogg and Y. K. Wong. To all of them I owe a debt of gratitude.

[2] Augustin Cournot, *Researches into the Mathematical Principles of the Theory of Wealth* (1838), Bacon's Translation, New York, §24.

[3] A. C. Pigou, "A Method of Determining the Numerical Value of the Elasticities of Demand," *Economic Journal*, xx (1910) 636–640; and *The Economics of Welfare*, 3rd ed., London (1929), Appendix II, pp. 780–786.

[4] Alex. Tschayanow, *Essays on the Theory of Labor* (in Russian), Moscow (1912). Quoted in Jacob Marschak, "Elastizität der Nachfrage," *Beiträge zur Ökonomischen Theorie*, Tübingen (1931), p. 53.

Lehfeldt published an estimate of the elasticity of demand for wheat.[5] Furthermore, Moore's own procedures were essentially at hand in 1891 in a book by two American business men, Arthur B. and Henry Farquahr,[6] and more especially in a paper by Professor Rodolfo Benini,[7] published in 1907, in which the latter went so far as to derive the demand for coffee in Italy as a function of its price and the price of sugar, by the method of multiple correlation; and in Dr. Marcel Lenoir's important book, *Études sur la formation et le mouvement des prix*, which was published in 1913.[8] But none of these predecessors of Moore attracted much attention, none covered so wide a field, and none succeeded so well in wringing fresh knowledge from the accumulated masses of data.

Moore's problem centered around this difficulty: "Economic theory," in the words of the late and lamented Professor Allyn A. Young, "has never professed to deal with the temporal succession or the spatial distribution of unique combinations of circumstances, while statistics has to deal, in the first instance, with nothing else."[9] If the form of the demand function and the interrelations of the variables entering into it and time were known, the difficulty to which Professor Young referred could be overcome; but they are not. Is there a way out?

Moore's contributions to the solution of this problem are three: (1) He restated the hypothetical, statical law of demand in a form admitting of concrete, inductive treatment; (2) he devised ingenious statis-

[5] "The Elasticity of Demand for Wheat," *Economic Journal*, XXIV (June, 1914), 212. For a comment on Professor Lehfeldt's procedure, see my *Statistical Laws of Demand and Supply with Special Application to Sugar* (University of Chicago Press, 1928), pp. 211–212.

[6] Arthur B. and Henry Farquahr, *Economic and Industrial Delusions* (New York: G. P. Putnam's Sons, 1891), pp. 206–208. See my *Der Sinn der statistischen Nachfragekurven*, Veröffentlichungen der Frankfurter Gesellschaft für Konjunkturforschung (edited by Dr. Eugen Altschul), Bonn, 1930, Appendix III.

[7] "Sull'uso delle formule empiriche nell' economia applicata," *Giornale degli Economisti*, XXXV (1907 II), 1052–1063. See also his "Una possibile creazione del metodo statistico: L'economia politica induttiva," *ibid.*, XXXVI (1908 I), 11–34. As far as I can determine, the first economist to call attention to Benini's work is Professor Umberto Ricci. See his "Elasticita dei besogni, della domanda e dell' offerta," *Giornale degli Economisti*, Aug. and Oct., 1924, 54. Fuller references to his work may be found in Professor C. Bresciani-Turroni's "Über die Elastizität des Verbrauchs ägyptischer Baumwolle," *Weltwirtschaftliches Archiv*, XXXIII no. 1 (Jan., 1931), 48; and in Professor Felice Vinci's scholarly paper on "L'elasticità dei consumi," *Rivista Italiana di Statistica* (Jan.–Mar., 1931), IX, 30–91.

[8] M. Lenoir, *Études sur la formation et le mouvement des prix*. Paris: M. Gerard and E. Brière, 1913.

[9] "English Political Economy," *Economica*, No. 22 (March, 1928), p. 10.

tical devices, such as the method of link relatives and the method of trend ratios, for handling the time variable, and adapted the method of multiple correlation to the same end; and (3) he succeeded in deducing for the first time the statistical demand curves for several important commodities, and in measuring their elasticities of demand. These curves, he was careful to point out, are not identical with the Cournot-Marshall demand curves, although they may approximate them under certain conditions. The statical demand curve, he argued, can only be approximated, but never realized in inductive investigations. The path which he has thus blazed has been followed by many workers, both at home and abroad.

Very recently, however, several other attacks on the problem of deriving the elasticity of demand from statistics have come to the forefront. Though they differ among themselves, they have this characteristic in common: they are attempts to derive the demand curve and the elasticity of demand from income data or from family budget data rather than from time series of prices and quantities.[10] They may be said to constitute a revival and a development of the point of view and method exemplified in Professor Pigou's *first* attack on this problem in 1910.[11] Among the economists and statisticians whose names are connected with these methods are Professor Ragnar Frisch[12] (1926),

[10] For a clear and suggestive statement of the relation between changes in income and changes in demand, see Vilfredo Pareto, *Cours d'économie politique*, II (1897), §§973—989.

[11] A distinction must be drawn between Pigou's first (1910) and second (1930) method of approach, because the latter is essentially a modification of the link relative approach. See "The Statistical Derivation of Demand Curves," *Economic Journal*, XL, no. 159 (Sept., 1930), 384–399. This paper contains several arithmetical errors, which, however, do not affect his main conclusions. I hope to discuss it elsewhere. Here I wish only to point out that what Professor Pigou calls his most serious criticism of the methods of link relatives and trend ratios, a criticism which appears to constitute the raison d'être of his own paper, namely, that in these methods "no explicit calculation is made of the probable error to which the results attained are subject" (p. 391), is not based on fact. Probable errors (or standard errors) in the least square sense can, of course, be derived for the elasticities of demand. In my previous publications I either derived them, or else indicated why it was not necessary or desirable to compute them. I am pleased to say that Professor Pigou has in a letter retracted this criticism.

[12] Ragnar Frisch, "Sur un problème d'économie pure," *Norsk Matematisk Forenings Skrifter*, Series 1, No. 16, 1926; and his latest book, *New Methods of Measuring Marginal Utility*, Beiträge zur ökonomischen Theorie, J. C. B. Mohr (Paul Siebeck), Tübingen, 1932. For a review of this book see my paper, "Frisch on the Measurement of Utility," *Journal of Political Economy*, L, no. 1 (February, 1933).

Professor Irving Fisher[13] (1927), Professor René Roy[14] (1930), and Dr. Jacob Marschak[15] (1931), not to mention the methodological works of Professors Ricci, Gini, Vinci, and of others.[16]

Both types of approach—those based on market data and those based on family budget data—have their peculiar limitations and advantages, both theoretical and practical. Some of the disadvantages of the market data approach are: (1) the difficulty of disentangling the dynamic factors, especially the time variable; (2) the difficulty of deriving, by its aid, the elasticity of demand for *classes* of commodities and services, such as clothing, housing, amusement, etc. Some of the limitations of the budget data approach are: (1) the impossibility of deriving the elasticities of demand of intermediate goods, or goods that are not consumed directly by human beings, such as hay, corn, rye, and wheat,[17] as well as iron, steel, copper, etc.; (2) the difficulty or impossibility of determining the standard error of the coefficient of elasticity; and (3) the difficulty of allowing for the effects of substitutes.

For the last few years, I have been engaged, on and off, in a study of the demands for various goods, in so far as they can be determined from market data. In this connection I have had occasion to work with time series of prices and quantities of varying degrees of reliability, and to experiment with different methods of procedures. This paper is essentially a summary and a comparison of some of the results that have been obtained by the various methods in so far as they relate to the

[13] Irving Fisher, "A Statistical Method for Measuring 'Marginal Utility' and Testing the Justice of a Progressive Income Tax," in *Economic Essays Contributed in Honor of John Bates Clark*, edited by Jacob H. Hollander, 1927. In a circular letter which accompanied the reprint of this paper, Professor Fisher writes: "Although the publication of my own method comes later, I had, in unpublished lectures, employed it at least as early as 1912. In an article entitled "Is 'Utility' the Most Suitable Term for the Concept it is Used to Denote?" in the *American Economic Review*, June, 1918, I referred to the intention of publishing this method; but publication was put off from year to year in the hope of first making a full statistical application."

[14] René Roy, "La demande dans ses rapports avec la répartition des revenus," *Metron*, VIII, 101–153, and "Les lois de la demande," *Revue d'économie politique*, July–August, 1931, pp. 1190–1218.

[15] Jacob Marschak, *Elastizität der Nachfrage*, J. C. B. Mohr, Tübingen, 1931. Marschak's monograph contains a good succinct analysis of the methods of Pigou, Tschayanow, Frisch, and Fisher, but not of Roy. His criticism of Frisch is, however, based on a misconception which I point out in my review of his book in the *Weltwirtschaftliches Archiv*, January, 1933.

[16] See references in note [7].

[17] Bread is quite a different commodity from wheat. When we buy a loaf of bread we buy milk, sugar, salt, wrapping, service, etc., in addition to wheat.

elasticity of demand. They invite comparison with the results that have been, or that can be, derived by the budget data approach.[18]

II. THE DATA

The commodities whose elasticities of demand will be compared in this study are nine farm crops, for each of which I have determined not only the shape of the demand curve, but also its rate of shift from 1875 to 1929. Table I is a list of these commodities with their farm values in 1915 and 1925.

TABLE I

AGRICULTURAL COMMODITIES WHOSE ELASTICITIES OF DEMAND WERE DETERMINED BY DIFFERENT METHODS, WITH THEIR FARM VALUES IN 1915 AND IN 1925[a]

Commodity	Farm value (In millions of dollars)	
	1915	1925
Corn	1,723	1,967
Hay	914	1,195
Wheat	942	958
Sugar[b]	473	677
Potatoes	222	604
Oats	560	566
Barley	118	127
Rye	45	36
Buckwheat	12	12
Total	5,009	6,142

[a] Source: Yearbooks of Agriculture for 1920 and 1926.

[b] Represents United States consumption multiplied by the wholesale price of refined sugar at New York. The data are from *Concerning Sugar*, loose-leaf service of the United States Sugar Manufacturers' Association.

My aim has been to derive the aggregate market demand curve for each crop in order to be able at some later time to bring it into juxtaposition with the corresponding market supply curve. This suggested the desirability of working with the combined demand curve of the wholesalers, dealers, and farmers. For the purpose of deriving such a demand curve the data that are required are "total disappearance" (production minus net exports, plus beginning stocks, and minus end stocks), and the average price at which all exchanges have taken place.

[18] For a discussion of the relation between the statistical demand curves and the demand curves of pure theory, see my *Statistical Laws of Demand and Supply*, pp. 15–34, and especially my *Der Sinn der statistischen Nachfragekurven*, pp. 11–39.

Since, however, such complete data are not available, it is necessary to approximate the quantity of the commodity demanded and the corresponding average price from such series as may be obtained.

For five commodities—buckwheat, corn, hay, oats, and potatoes—the United States Department of Agriculture estimates for production have been used. The net exports or imports of these crops are small, seldom exceeding five per cent of production. There are no data on stocks; but there is reason to believe that the fluctuations in the carry-over of each of these commodities from year to year are relatively small and do not have an important effect on the average price. Where net exports are of appreciable amounts, such as those for barley and rye, they have been subtracted from production estimates to obtain a "consumption" series. The consumption data for sugar represent production plus imports.

For wheat, a total disappearance series for food, feed, and loss, since 1896 has been derived from the estimates of stocks and seed requirements as given in the U.S.D.A. tables of wheat supply and distribution. These estimates are not available for the period preceding 1890. These total disappearance figures, though they are admittedly incomplete, nevertheless appear to give a closer indication of the quantity of wheat demanded in any one year than does the simpler series of production minus net exports.

The basic production estimates are not entirely reliable. A comparison of the Department estimates with census figures shows wide discrepancies in some years. Thus, for buckwheat, the Department estimate is 21 per cent higher than the census figure in 1909, and 11 per cent higher in 1924; for potatoes the estimates are 11 per cent higher than the census data in 1919, and 19 per cent higher in 1924; while for rye similar percentage differences are 20 and 18 for the census years 1909 and 1924. This comparison is not to be interpreted as suggesting that the census estimates are necessarily more accurate than those of the U. S. D. A. The U. S. D. A. revisions for recent years (which were published after our computations had been completed) show, however, a closer agreement with the census data.

Most of these discrepancies, however, throw more doubt on the actual *level* of the series used than on the year-to-year changes. If, through an error in determining the level of production in any one year, the true trend of production has been distorted, this will affect the rate of shifting of the demand curve derived from the data, but not necessarily its shape or its elasticity. The latter depends primarily on the year-to-year fluctuations of the quantities and the related prices. In some instances, however, a special study of the data also led to serious doubts as to their *relative* accuracy in certain years. These ob-

servations were, therefore, omitted from the computations. (See note b to Tables III and IV.)

For all commodities except sugar and barley, the price series used is the December 1 farm price. This series was adopted primarily because it is available for the entire period covered by this study, and because it is an average of all classes and grades and therefore approximates our ideal figure—the average price at which the commodity changed hands in the country as a whole. The correlations between the December 1 price and the crop year average of monthly farm prices since 1909, the first year for which the latter are available, are very high, indicating that the December 1 price is typical of the price for the year as a whole.[19]

For barley, it was found that the Chicago price (an August–July average of quotations for choice to fancy or fair to good malting grades in Chicago) and the farm price, instead of showing the customary general agreement, differed definitely as to direction of trend during the first period, 1875–1895. This seemed to be due to a decided shift in regions of barley production. The average farm prices for the early years of the period were heavily weighted by the high prices of the areas of New York and California; those for the later years by the low prices of the Chicago region. The Chicago price seemed, therefore, to be more representative of the general trend of barley prices throughout the country than the farm price.

The average New York wholesale price of refined sugar was taken because New York was the dominant sugar market during the period covered by this investigation; prices in all markets throughout the United States were fixed by the New York price.

All price series have been deflated by the Bureau of Labor Statistics all-commodity index of wholesale prices (1913 = 100), as extrapolated by Carl Snyder back to 1875. December 1 farm prices have been deflated by the index number for December. The Chicago price for barley has been deflated by the average index for the year beginning August. The New York wholesale price (yearly average) for sugar has been deflated by the average index for the year beginning January 1.

The data are expressed in terms of bushels per capita and cents per bushel for all the commodities except sugar (pounds per capita, and cents per pound), hay (tons per animal unit, and dollars per ton), and oats (bushels per animal unit).

[19] Charles F. Sarle, "Reliability and Adequacy of Farm-Price Data," *Bulletin No. 1480, U.S.D.A.*, March, 1927.

III. THE METHODS

But the period from 1875 to 1929, which is covered by this study, saw too many changes in our economic history to constitute a homogeneous period for our purposes. I have, therefore, broken it up into three parts as follows: (I) 1875–1895, the period of general falling prices; (II) 1896–1914, the period of general rising prices; and (III) 1915–1929 (omitting 1917–1921), the period of the world war and reconstruction; and made a study of the data for each part, with the view of obtaining a better insight into the changes that have taken place in the shape of the demand curves and in the direction and rate of their shifting.

To determine the effect of the different methods on the elasticity of demand, I have experimented with trend ratios, link relatives, multiple correlations, and combinations of them, and have worked with total consumption and money prices (unadjusted data) as well as with per capita consumption and "real" prices (adjusted data). Also, in order to throw light on still other problems, I have taken first consumption and then price as the dependent variable, and have used other types of regression equations as well.

Here I shall concern myself primarily with the adjusted data, and only with the regressions in which quantity is the dependent variable. More specifically, I shall consider only the methods and the equations outlined in Table II.

TABLE II

METHODS AND TYPES OF EQUATIONS USED IN DETERMINING THE ELASTICITIES OF DEMAND FOR SELECTED COMMODITIES FROM ADJUSTED DATA

I. *The Time-regression Method*	
Type of demand equation assumed	
(1) $x=a+by+ct$	$a>0, b<0, c\gtrless 0$
(2) $x=Ay^{\alpha}e^{\beta t}$	$A>0, \alpha<0, \beta\gtrless 0$
II. *The Method of Trend Ratios*[a]	
Type of demand equation assumed	
(3) $X=a+bY$	$a>0, b<0$
(4) $X=AY^{\alpha}$	$A>0, \alpha<0$
III. *The Method of Link Relatives*[a]	
Type of demand equation assumed	
(5) $X=a+bY$	$a>0, b<0$
(6) $X=AY^{\alpha}$	$A>0, \alpha<0$

[a] Capital letters indicate trend ratios or link relatives in contrast with the small letters which indicate actual quantities and prices.

These types of equations were found generally to give a good de-

scription of the data relating to the nine commodities listed in Table I.[20] But before proceeding to compare the coefficients of elasticity, it is necessary to familiarize ourselves with the properties of the various equations and with the differences between the various methods.[21]

The time-regression method.—Type (1) represents a straight surface or plane, type (2) a curved surface. We may, however, transform (2) into a straight surface by writing it in the form

$$\log_e x = \log_e A + \alpha \log_e y + \beta t.$$

When we fit type (1) to data, we are assuming that the demand curve is a straight line with slope b, and that it shifts its position upward (or downward) to the right by c units per annum. According to this assumption, the coefficient of partial elasticity of demand, η, is equal to

$$\eta_{xy.t} = \frac{\partial x}{\partial y}\frac{y}{x} = b\frac{y}{x}. \tag{7}$$

Conversely, when we fit type (2) to data, we are assuming that the demand curve has a constant elasticity, α, at every point, this elasticity being

$$\eta_{xy.t} = \frac{\partial x}{\partial y}\frac{y}{x} = \frac{\partial \log_e x}{\partial \log_e y} = \alpha, \tag{8}$$

and that the annual demand curve shifts not by a constant absolute amount, but by a constant relative amount equal to β, for $\beta = \frac{1}{x}\frac{\partial x}{\partial t}$.

The method of trend ratios.[22]—The trend ratios are defined as $X = x/T_x$ and $Y = y/T_y$, where T_x and T_y are functions of time representing respectively the trends of x and y. Substituting in type (3) and multiplying both sides by T_x, we obtain

$$x = aT_x + b\frac{T_x}{T_y}y. \tag{9}$$

[20] The exceptions are indicated in the notes to Tables III and IV.

[21] For a discussion of the *rationale* of these methods, see my *Der Sinn der statistischen Nachfragekurven*, Chapters I and II, and my *Statistical Laws of Demand and Supply*, pp. 32–34.

[22] The use of ratios-to-trend (as well as deviations-from-trend) for eliminating long-time factors, and thus isolating the short-time fluctuations, was suggested and described by Warren M. Persons in 1910, who mentions their previous application in J. P. Norton's *Statistical Studies in the New York Money Market*, New York: Macmillan Company, 1902. See his paper, "The Correlation of Economic Statistics," *Quarterly Publications of the American Statistical Association*, XII, no. 92 (Dec., 1910), 317–318.

For any given year for which T_x and T_y are known, this is the equation of a straight line in x and y, since we assume T_x and T_y to be time series not depending on the values of x and y.

The slope of the line, namely

$$b\frac{T_x}{T_y},$$

may, of course, vary from year to year, depending on the ratio of T_x to T_y.

By computing the demand curve (9) for each year and comparing the results, we can observe the way in which the demand curve has shifted (upward or downward) according to assumption (3). But since the curve generally changes its slope as it shifts to the right, the different points on it shift at different rates. Consequently, it is very difficult to summarize the rate of shift of the curve as a whole, although we can easily measure the rate of shift of a particular point on it.

The elasticity of demand for any given year is

$$\eta = \frac{dx}{dy}\frac{y}{x} = b\left(\frac{T_x}{T_y}\right)\frac{y}{x} = b\frac{Y}{X}. \tag{10}$$

This shows that if assumption (3) is adopted, η becomes a constant for any given point on the (X, Y) curve; and that it changes with time only because the point on the (X, Y) demand curve changes with time. If, however, we consider η as a characteristic of any given point on the (x, y) curve, it will change with time even though the point (x, y) itself does not change.

When we use type (4), we are assuming that the demand curve has a constant elasticity α at every point, both on the (X, Y) curve and on the (x, y) curve, regardless of whether the curve is written in the trend ratio form or in terms of absolute quantities and prices. For, by substituting x/T_x for X and y/T_y for Y in (4) we get

$$x = A\frac{T_x}{T_y^{\alpha}}y^{\alpha}, \tag{11}$$

α being the elasticity of demand.

A general idea of the rate of shift of the demand curve may be obtained by computing (11) for each year and comparing the results. The exact measurement of the rate of shift is, however, subject to the same difficulties as those encountered above in connection with (9).

The method of link relatives.—The transformation of the link relative equations (5) and (6) into equations expressed in terms of actual prices and quantities is similar to the transformation of the trend ratio equa-

tions discussed above. All we have to do, for example, to transform (5), is to substitute in it x_i/x_{i-1} for X_i, and y_i/y_{i-1} for Y_i, thus obtaining

$$x_i = a(x_{i-1}) + b\frac{x_{i-1}}{y_{i-1}}y_i. \tag{12}$$

This is the equation of a straight line in x_i and y_i, if the preceding year quantity x_{i-1} and the preceding year price y_{i-1} are taken as known constants.

For any given year, the slope of this line is constant, namely

$$b\frac{x_{i-1}}{y_{i-1}}.$$

The elasticity of demand, namely

$$\eta = \frac{dx_i}{dy_i}\frac{y_i}{x_i} = b\frac{x_{i-1}}{y_{i-1}}\frac{y_i}{x_i} = b\frac{Y}{X} \tag{13}$$

varies from point to point on the (x_i, y_i) curve and also on the (X_i, Y_i) curve. At any given point on the (X_i, Y_i) curve, it is constant over time. But at a given point on the (x_i, y_i) curve it varies with time because the parameters x_{i-1} and y_{i-1} vary with time.

By the same procedure we may transform (6) into an equation in terms of actual prices and thus obtain

$$x_i = A\frac{x_{i-1}}{y_{i-1}^{\alpha}}y_i^{\alpha}. \tag{14}$$

The slope of this curve varies from point to point, but the coefficient of elasticity of demand is constant at every point, being equal to α. Thus, assumption (6) entails a constant elasticity over time, both for the (X_i, Y_i) curve and the (x_i, y_i) curve.

Formula (13) for computing the elasticity of demand from a straight-line demand curve has been criticized by Professor Luigi Amoroso.[23] Lack of space does not permit a discussion of his contentions in this paper. I hope to show in another publication, however, that his contentions are not well taken.

The rates of shift of (12) and (14) may be studied in a manner analogous to that suggested above for (9) and (11).

IV. ILLUSTRATION

As a concrete illustration of the various procedures listed in Table

[23] "Le equazioni differenziali della dinamica economica," *Giornale degli Economisti*, February, 1929, pp. 68–79.

II, I shall briefly analyze the demand for sugar in the United States during 1896–1914 by the various methods.

Time-regression method.—Figure 1 is a graphic representation on a logarithmic scale of the per capita consumption and the real prices of sugar during this period, with their computed trends. (The value per capita series will not concern us in this study.) It will be observed that there is a rather high inverse correlation between the long-time swings of the two series, as well as between their year-to-year movements.

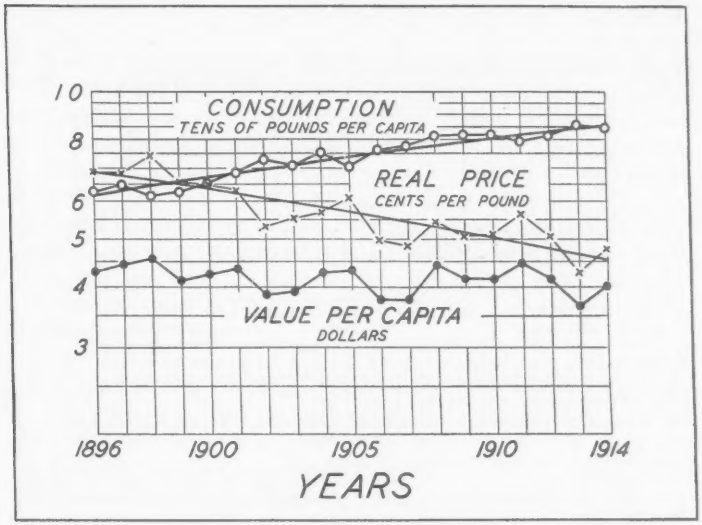


FIGURE 1.—The basic adjusted annual data used in deriving the elasticity of demand and the rate of shift of the demand curve for sugar in the United States, 1896–1914, with the computed trends of the real prices and the per capita consumption.

The existence of this two-fold correlation is brought out very clearly by the general negative direction of the scatter in Figure 2A, in which the per capita consumption is plotted against the adjusted prices. The gross coefficient of correlation between the two series is $r = -0.921$. It is the negative correlation between the two series as they stand, no allowance having been made for their respective secular movements.

A closer inspection of Figure 2A also brings out the negative year-to-year correlation. On the average, an increase in the price of sugar between any two years is accompanied by a decrease in consumption, and vice versa. If we fit to the data a regression of type (1), we elimi-

nate the effect of *time* (t), which may be thought of as a catch-all for certain disturbing factors—say, the changing taste for sugar. We thus get line D_1D_2, drawn through the means of the observations. This is the *net* regression of consumption on price. As seen from Figure 2A, the slope of this line is fairly typical of the slopes of the lines connecting the successive observations. It gives the relation between that part of the per capita consumption which is linearly independent of time, and that part of the real prices which is linearly independent of time. It may be taken as a good approximation to the demand curve for sugar.

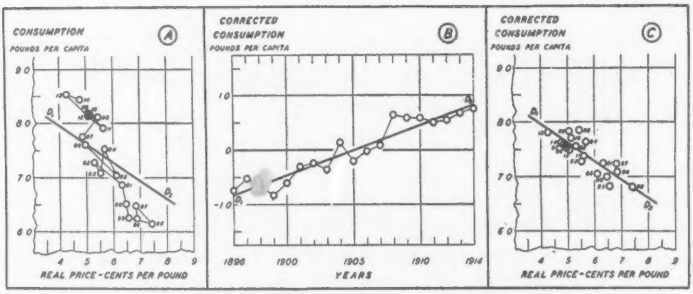


FIGURE 2.—Three aspects of the per capita demand for sugar during the period 1896–1914, under assumption (1).

A.—Relation between the per capita consumption and the real prices when the data are not corrected for the effects of "time," with the demand curve D_1D_2 which results when such correction is made.

B.—Relation between the per capita consumption and time, when the former is corrected for the effects of changes in the real price.

The slope of line D_1D_2, fitted to the corrected data, represents the mean rate of shift of the per capita demand curve D_1D_2.

C.—The per capita demand curve for sugar, 1896–1914, with the corrected observations. Line D_1D_2 shows the relation between that part of the per capita consumption which is independent of time, and that part of the real price which is independent of time.

In the scatter, however, the ordinates, but not the abscissas, have been corrected for secular changes.

In view, however, of the high inverse correlation existing between the two series as they stand, it may be asked, "Why consider time at all? Is not the decline in the real price of sugar the *cause* of the increase in the per capita consumption? Why not take the ordinary gross regression (not shown on the graph) as our demand curve?"

The answer is that the gross correlation is due, in part at least, to the long-time secular movements of the per capita consumption and the real prices, and that the existence of such movements in any series

ought to make us suspect the existence of disturbing factors which might produce spurious correlation, or which might give rise to long-time, irreversible demand or supply curves. One obvious factor which may cause a spurious correlation is the growing taste for sugar which we assumed to change slowly with time. We must not, therefore, accept the *gross* regression of consumption on price as a *bona fide* short-time *reversible* demand curve.[24] We must instead seek to determine the *net* regression between per capita consumption and real prices, i.e., the relation between that part of the per capita consumption which is independent of time and that part of the real price which is independent of time.

To determine whether, and how far, we can reduce the scatter about the demand curve D_1D_2 by assuming that it has shifted its position from year to year, we take the differences between the computed and the observed consumption (i.e., the vertical deviations) and plot them as a function of time. The results are shown in Figure 2B. The line D_1D_3 fitted to these deviations shows the trend in consumption that cannot be accounted for by a trend in price. The positive slope of this line indicates that the demand curve D_1D_2 has shifted, on the average, upward and to the right.

When allowance is made for this upward shift, we obtain the reduced scatter of Figure 2C.

The three diagrams shown in Figure 2 are, however, only different aspects of the three-dimensional relation shown in Figure 3. In Figure 3, each observation is shown in three dimensions. (The significance of the black and the white circles will be explained later.) For example, the first observation shows that in 1896, the real price was 6.9 cents a pound, and the consumption was 62 pounds per capita.

The orthogonal projections of the observations (circles) on the vertical plane XOT give the fluctuations in the per capita consumption of sugar which were shown in Figure 1. The corresponding projections on the horizontal plane YOT give the fluctuations in the real price of sugar, which were also shown in Figure 1. The corresponding projections on the last (1914) plane give the scatter diagram of consumption on price, which was shown in Figure 2A.

The plane $D_1D_2D_3D_4$ (which will be called D_1D_4) is the demand surface (1) which was fitted to the same data. (The curved surface $D_1'D_4'$ will be explained later.) This surface is a graphic representation of the equation

[24] A reversible demand curve is such that when the price falls from y_2 to y_1, the quantity sold will increase from x_1 to x_2; and when the price rises from y_1 to y_2, the quantity sold will fall from x_2 to x_1, the original amount.

$$(15) \qquad x = 92.90 - (3.341 \pm 1.009)y + (0.9197 \pm 0.1521)t,$$

the origins of x and y being at 0,0 and the origin of t being at 1905.[25] The observations which lie above this plane are represented by black circles. Those which lie below the plane are represented by single-barred circles($\ominus$). These falling between the two planes are represented by double-barred circles ($\oplus$).

This equation means that, in any given year, an increase of 1 cent in the real price of sugar is associated with a decrease of 3.34 pounds in the per capita consumption. This relation is indicated by the slope

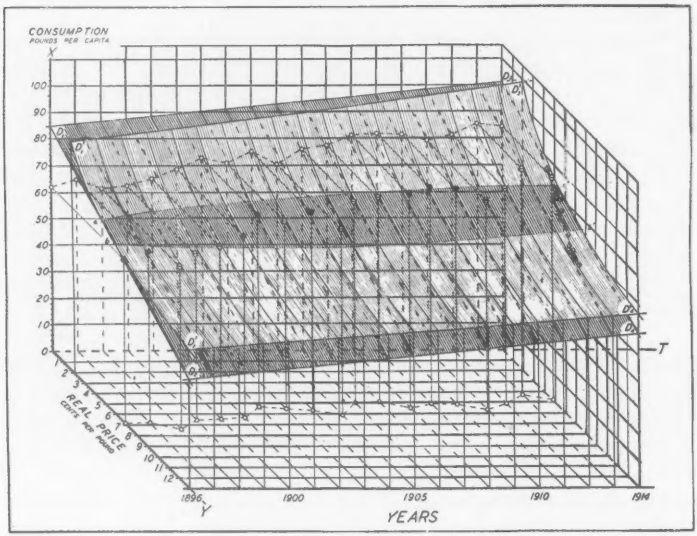


FIGURE 3.—The per capita demand for sugar, 1896–1914, derived by the time-regression method, and illustrating assumptions (1) and (2). (See equations (15) and (16).)

of the demand curve D_1D_2. It is this demand curve which is shown in Figure 2A and 2C.

But even if we fix the price of sugar, the per capita consumption still shows an increase of 0.9197 pounds per annum. This rate of shift of the demand is shown by the slope of the line D_1D_3.

The fit of the plane D_1D_4 to the observations is very good. The quadratic mean error is only ± 1.82 pounds, which means that approximately two-thirds of the observations (points) fall within a distance of

[25] The figures preceded by $\pm$ are *standard* errors, not probable errors.

1.82 pounds above and below the plane. The coefficient of correlation between the observed and the computed consumption is 0.976.

The following example will illustrate the procedure for computing the elasticity of demand from (15): In 1904 the real price was 5.68 cents. Substituting this value for y in (15) and putting $t = -1$ in the same equation (since the origin of t is at 1905), we obtain for the theoretical per capita consumption 73.00 pounds. Substituting these values of x and y in (7), we obtain

$$\eta_{xy.t} = -0.2600.$$

This means that if the real price which prevailed in 1904 had been decreased (or increased) by 1 per cent, and *if the demand curve had remained fixed for one year*, there would have been an increase (or a decrease) of 0.26 of one per cent in the annual per capita consumption.[26]

But the demand curve has not remained fixed. As stated above, it has shifted its position upward and to the right at the rate of 0.9197 pounds per capita per annum.

The foregoing changes may also be described in relative terms by means of a demand function of type (2). The curved surface $D_1' D_4'$ is a graphic representation of such a function. Its equation is

$$x = 117.5y^{-(0.2717 \pm 0.0844)} e^{+(0.0124 \pm 0.0022)t}, \tag{16}$$

where x, y and t have, respectively, the same meaning and the same origins as in equation (15).

This equation tells us that when the real price of sugar is held constant, the per capita consumption increases at the momentary rate of 1.24 per cent per annum, for

$$\frac{1}{x}\frac{\partial x}{\partial t} = 0.0124 \pm 0.0022. \tag{17}$$

This is shown graphically by the slope of the curve $D_1'D_3'$.

The elasticity of demand is

$$\eta_{xy.t} = -(0.2717 \pm 0.0844). \tag{18}$$

This means that if the real price which prevailed in any given year had been decreased (or increased) by 1 per cent, *and if the demand curve had remained fixed for one year*, there would have been an increase (or decrease) of approximately 0.27 of 1 per cent in the annual per capita consumption. This coefficient is not very likely to be numerically less than

[26] The elasticity of demand is a function of the *time interval* between successive observations on which the demand function is based. Thus, the use of annual data might lead to a demand curve which is quite different from that based on monthly figures for the same series.

0.18, nor greater than 0.36. The value of this coefficient is constant for every point on the demand curve.[27]

The fit of equation (16) is just as good as that of equation (15). The quadratic mean error[28] of the observations is

$$\epsilon = \pm 0.01150 \text{ (in logs)}. \tag{19}$$

The antilogarithms of this number are 0.97 and 1.03. This means that approximately two-thirds of the observation points fall within a distance of 3 per cent above the plane and 3 per cent below the plane. The coefficient of multiple correlation is 0.974.

A glance at Figure 3 shows that the curved surface $D_1' D_4'$ intersects the straight surface D_1D_4 twice: along the curved line aa' as it dips below the straight plane, and along the curved line bb' as it emerges from under it. Unlike the straight surface D_1D_4, the curved surface $D_1' D_4'$ does not cut the vertical plane of reference XOT, but is asymptotic to it. In order however, better to show its relation to the straight surface, it was terminated at $D_1' D_3'$, which gives the consumption (x) for each year on the assumption that the price is kept constant at $y=2$ cents.

A comparison of the two surfaces discloses the interesting fact that within the range of the observations there is no appreciable difference between the two surfaces.

The method of trend ratios.—The equations to the trends of the per capita consumption (measured in pounds) and the real price of sugar (measured in cents) from 1896–1914 are, respectively

$$T_x = 73.85 + 1.356t \tag{20}$$

and

$$T_y = 5.700 - 0.1305t, \tag{21}$$

the origin for each curve being at 1905.

[27] In this curve, as well as in all the other curves listed in Table II, the coefficient of elasticity of demand is independent of the rate of shift of the demand curve. Nevertheless, the italicized condition is necessary in order to emphasize the fact that it takes time for consumption to adjust itself to a price change. When the demand curve is derived from annual data, the necessary time interval is generally one year, or one season.

[28] Since the curve was fitted in the logarithmic form, the quadratic mean error is

$$\epsilon = \sqrt{\frac{\sum[(\log x)_o - (\log x)_c]^2}{n-m}},$$

where $(\log x)_o$ is the observed value of the logarithm, $(\log x)_c$ the computed value, n the number of sets of observations, and m the number of parameters in the equation.

The trend ratios of consumption and the corresponding trend ratios of price are negatively correlated with each other, the coefficient of correlation being $r = -0.623$.

The law of demand in the trend ratio form is

$$X = 1.251 - (0.2512 \pm 0.0766)Y, \tag{22}$$

its quadratic mean error being $\epsilon = \pm 0.0246$ units.

In terms of actual prices and quantities, the equation is, by (9),

$$x = 1.251T_x - 0.2512\frac{T_x}{T_y}y. \tag{23}$$

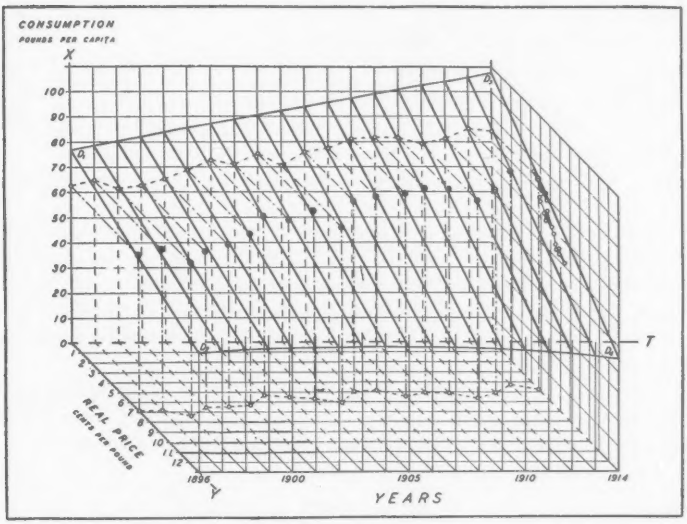


FIGURE 4.—The per capita demand for sugar, 1896–1914, derived by the method of trend ratios, and illustrating assumption (3). (See equations (22) and (23).)

Figure 4 is a graphic representation of this function. Each heavy line of the type D_1D_2 represents the average demand curve for the year in question. The underlying theoretical curves are probably subject to rather pronounced seasonal fluctuations, but these cannot be determined from annual data.

Curve D_1D_3 or D_2D_4 measures the rate of shift of the demand curve D_1D_2, which is implied by equation (23) when the trends of x and y

are, respectively, the straight lines (20) and (21). These curves should be compared with the corresponding curves of Figure 3.

It is important to observe that while the curve D_1D_3 shows that the demand curve shifted upward throughout the entire period, the curve D_2D_4 shows that it shifted upward until 1907 and then downward.

When the trend ratios of x and y are given their mean values ($X = 1.0000$ and $Y = 1.0003$), then the elasticity of demand is, by (10)

$$\eta = \frac{dx}{dy}\frac{y}{x} = b\frac{Y}{X} = -0.2512\frac{1.0003}{1.0000} = -0.2512. \tag{24}$$

That is to say, when both consumption and price are at their normal (trend) levels, a change of 1 per cent in price, if maintained for the period in question (one year) would, on the average, be associated with a change of only 1/4 of one per cent in the annual consumption. This value of the elasticity of demand happens to be almost identical with the corresponding values derived from equation (15) and equation (16).

If instead of fitting a straight line, we fit an equation of type (4), we obtain

$$X = 0.9989Y^{-(0.2575\pm 0.0774)}. \tag{25}$$

The elasticity of demand and the other results derived from this equation are similar to those yielded by (22). A graph of this function would resemble that of Figure 4, except that the straight lines would be replaced by curves of constant elasticity (hyperbolas). Within the immediate range of the data, there is no significant difference between the two sets of equations.

The method of link relatives.—The link relatives of consumption and the corresponding link relatives of the prices are negatively correlated with each other, the coefficient of correlation being $r = -0.612$.

The law of demand in the link relative form is

$$X = 1.269 - (0.2551 \pm 0.0824)Y, \tag{26}$$

its quadratic mean error being $\epsilon = \pm 0.0335$ units.

In terms of actual prices and quantities, the equation is, by (12)

$$x_i = 1.269(x_{i-1}) - 0.2551\frac{x_{i-1}}{y_{i-1}}y_i. \tag{27}$$

Figure 5 is a graphic representation of this function. It corresponds to Figure 4.

The elasticity of demand when the link relatives are given their mean values ($X = 1.018$ and $Y = 0.9849$) is by (13)

$$\eta = -0.2469. \tag{28}$$

I interpret this result to mean that when the per capita consumption for any year is "normal," or 1.8 per cent above the consumption for the previous year, then an increase (or decrease) of 1 per cent in the price will bring about a decrease (or increase) of 1/4 of one per cent in consumption.

If we fit an equation of type (6), we obtain

$$X = 1.012Y^{-(0.2447\pm0.0789)}. \tag{29}$$

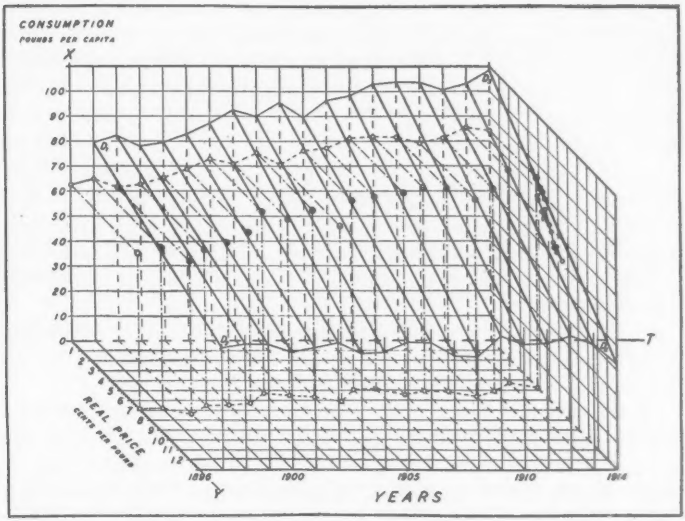


FIGURE 5.—The per capita demand for sugar, 1896–1914, derived by the method of link relatives, and illustrating assumption (5). (See equations (26) and (27).)

The elasticity of demand and the other results derived from this equation are similar to those yielded by (22). A graph of this function would resemble that of Figure 5, except that the straight lines would be replaced by curves of constant elasticity (hyperbolas). Within the immediate range of the data, there is no significant difference between the two sets of equations.

V. FINDINGS

The concrete illustrations which I have just given show how the shape (elasticity) of a demand curve and its rate of shift can be approximated by various methods. Applying these methods to the statistics of the nine commodities listed in Table I, I have deduced the elasticities of demand and the rates of shift of the demand curves of each

of these commodities for each of the periods: (I) 1875–1895, (II) 1896–1914, and (III) 1915–1929 (omitting 1917–1921). Of these two aspects of demand curves, however, only the former (elasticity) concerns us here. The latter (shifting) has received a fuller treatment in another study.[29] But before proceeding with a comparison of the results, we must dispose of two questions: (1) At what point on each demand curve shall the elasticity of demand be computed, for the purpose of comparison? and (2) What is the standard error of the elasticity of demand?

(1) The convention which I adopted for comparing elasticities that vary from point to point on the fitted demand curve was to consider *those points on two different demand curves (or surfaces) as comparable whose coordinates contain the mean values of their independent variables.* In equations of types (1) and (2) the independent variables for each period whose means we need are price and time; in equations of types (3) and (4), the independent variable whose mean we need is the trend ratios of the real prices; and in equations of types (5) and (6), the corresponding independent variable is the link relatives of real prices. It is believed that this convention will facilitate the inter-period and the intra-period comparisons of the elasticities of demand derived by different methods.

An experiment shows that the coefficients of elasticity corresponding to these *mean* values do not differ significantly—at least as far as corn and sugar are concerned.

(2) When the coefficient of elasticity of demand is constant at every point on the demand curve, and when the curve is fitted by the method of least squares, the standard error of that coefficient can be determined by a well-known least square procedure.[30] It is these standard errors which are attached to the constant coefficients of elasticity of this study. But when the coefficient of elasticity varies from point to point on the demand curve, its standard error is not given directly by the method of least squares, but must be derived from the standard errors of the parameters, and the variables on which it depends. Its exact value cannot be determined. An upper limit to it has been derived.[31] It is this limiting value which has been attached to the elas-

[29] "The Shifting Demand for Selected Agricultural Commodities, 1875–1929," read before the Joint Meeting of the American Statistical Association with the American Farm Economic Association, Washington, D. C., December 30, 1931, and published in the *Journal of Farm Economics*, XIV, no. 2 (April, 1931), 201–227.

[30] See my "The Standard Error of a Forecast from a Curve," *Journal American Statistical Association*, June, 1930.

[31] See my paper, "The Standard Error of the Coefficient of Elasticity of Demand," *Journal of the American Statistical Association*, XXVIII, no. 181 (March, 1933), pp. 64–69.

ticities of demand of equations (1), (3) and (5). These standard errors are not, therefore, strictly comparable with those of the constant elasticity equations (2), (4) and (6). No standard error is given for those elasticities which are derived from demand curves in which one or more of the parameters was obtained by a method other than the method of least squares.

Table III is a comparison of the elasticities of demand derived by different methods for 8 of the 9 commodities listed in Table I. It has been found impracticable to make such a comparison for wheat. Suffice it to say, however, that the demand for wheat is highly inelastic, the coefficient of elasticity being approximately of the order -0.01 for the second period, and -0.27 for the third period.[32] The results shown in Table III admit of comparison vertically by periods, and horizontally by methods used in any one period. We are primarily interested in the latter comparison.

Table IV is designed to indicate the goodness of fit of the various demand equations from which the elasticities of Table III have been computed. It gives for each demand equation the coefficient of correlation between the observed values of the dependent variable and the corresponding values computed from the equation. Thus the first entry in Table IV means that the coefficient of multiple correlation between the per capita consumption of corn on the one hand and real price and time on the other, when the demand equation is assumed to be of type (1), is $R_{xy \cdot t}=0.897$. This is also the simple correlation between the observed consumption series and that computed from (1). All figures to which no signs have been prefixed are either coefficients of multiple correlation or indexes of correlation. In comparing the coefficients of Table IV, allowance must be made for the different number of parameters of the various equations.[33]

The data on which Table III and Table IV are based have been adjusted for population growth and the purchasing power of money. It may be added, however, that substantially the same results are obtained by using unadjusted data, i.e., actual consumption and money prices.

[32] But since the demand curve for wheat is practically parallel to the axis of price, a small change in the slope of the curve, due to sampling fluctuations of the data, is associated with a large change in the coefficient of elasticity. The standard errors of the coefficients are, therefore, also large. There is no reasonable doubt, however, about the extreme *inelasticity* of the demand for wheat. For the equations to the demand curves and the related constants when per capita disappearance is used, see "The Shifting Demand for Selected Agricultural Commodities, 1875–1929," pp. 220 and 224.

[33] For the formula see my *Der Sinn der statistischen Nachfragekurven*, pp. 52–53, note 9.

A study of these tables suggests the following observations:

1. For any one commodity in any one period, there is close agreement between the elasticities of demand yielded by the various methods, notwithstanding the fact that the demand curves from which the elasticities are computed are not equally satisfactory for summarizing the relations involved. This is clearly seen from a comparison of corresponding rows in the two tables.

2. The standard errors of the coefficients of elasticity of demand are all relatively large, generally affecting the accuracy of the first significant figure. This makes it impossible to say with a high degree of probability whether the difference between any two elasticities is significant. It is better, however, to have a numerical, though approximate, value for the elasticity of demand of a commodity than none at all.

3. With the possible exception of buckwheat (first and third periods), the demand for all of the eight commodities is inelastic (i.e., $|\eta|<1$). A large supply of any one of these commodities is, therefore, worth less than a small supply, under normal conditions. This finding is supported by all the different methods.

4. The elasticities of demand for some commodities have remained remarkably constant from period to period. Thus, the elasticity of demand for sugar has been of the order -0.3 or -0.4 during each period. The elasticities of demand of other commodities have, however, fluctuated somewhat from period to period. Thus the elasticity of demand for corn seems to have decreased (numerically) and that for buckwheat seems to have increased (numerically) during the last period. But the standard errors of these coefficients are entirely too large to justify much confidence in these changes.

5. The elasticities of demand yielded by the time regression method and the trend-ratio method are generally in closer agreement with each other than they are with the corresponding elasticities derived by the link relative method. The latter method gives considerably higher numerical values for the coefficients of elasticity in the case of corn and barley in each of the three periods. There is need for a series of experiments with artificial series in order to explain the apparently erratic results yielded by the link relative method.

6. The fact that one method yields a lower correlation between the variables in question as compared with another does not necessarily mean that the elasticity of demand derived by it is materially different from that yielded by the method which gives the better description of the data. Compare, for example, the elasticities of demand for sugar, first period, with the corresponding coefficients of correlation.

TABLE III

ELASTICITIES OF DEMAND FOR SELECTED COMMODITIES, CLASSIFIED ACCORDING TO THE PERIOD COVERED BY THE DATA AND THE TYPE OF DEMAND EQUATION ASSUMED. ADJUSTED DATA

(Negative Values Throughout)

Commodity and period[b]		Type of demand equation assumed (See Table II)					
		(1)	(2)	(3)	(4)	(5)	(6)
Corn[c]	I	0.72±0.19	0.77±0.11	0.74±0.19	0.76±0.11	0.84[d]	0.84[d]
	II	0.60±0.11[e]	0.70±0.066[f]	0.57±0.099	0.68±0.067	0.74±0.15	0.77±0.074
	III	0.48±0.20	0.49±0.16	0.47±0.088	0.48±0.15	0.81±0.32	0.68±0.19
Hay[g]	I	0.72[h]	0.72[h]	0.78[d]	0.78[d]	0.67±0.22	0.69±0.16
	II	0.62±0.19[e]	0.69±0.16[f]	0.64±0.19	0.66±0.15	0.77±0.28	0.74±0.18
	III	0.46±0.27[e]	0.43±0.20[f]	0.62±0.35	0.62±0.28	0.53±0.33	0.56±0.23
Sugar	I	0.36±0.16	0.38±0.12	0.37±0.15	0.40±0.12	0.31±0.23	0.31±0.18
	II	0.26±0.093	0.27±0.084	0.25±0.087	0.26±0.077	0.25±0.094	0.24±0.079
	III	0.34±0.11[i]	0.31±0.077[j]	0.27±0.12	0.28±0.093	0.28±0.12	0.29±0.068
Potatoes	I	0.68±0.14[e]	0.66±0.056[f]	0.64±0.16	0.66±0.037	0.68±0.25[k]	0.71±0.042[k]
	II	0.61[l]	0.61[l]	0.55±0.13	0.59±0.069	0.59±0.17[k]	0.62±0.043[k]
	III	0.32±0.083	0.31±0.029	0.31±0.084	0.31±0.027	0.25±0.12	0.27±0.028
Oats[g]	I	0.54±0.16[e]	0.57±0.12[f]	0.59±0.18	0.62±0.14	0.47±0.20	0.64±0.13
	II	0.77±0.24	0.80±0.14	0.70±0.22	0.73±0.13	0.80±0.31	0.80±0.12
	III	0.60±0.55	0.56±0.46	0.57±0.50	0.54±0.42	0.79±0.69	0.71±0.48
Barley	I	0.17±0.13	0.16±0.12[f]	0.13±0.12	0.13±0.11	0.31±0.18	0.31±0.14
	II	0.18±0.11[e]	0.22±0.11[f]	0.20±0.13	0.22±0.12	0.24±0.17	0.23±0.13
	III	0.53±0.42[e]	0.39±0.24[f]	0.41±0.26	0.42±0.20	0.59±0.50	0.52±0.30
Rye	I	0.34±0.21	0.36±0.18	0.34±0.20	0.36±0.17	0.42±0.30	0.42±0.21
	II	0.36±0.14[e]	0.44±0.15[f]	0.43±0.17	0.43±0.14	0.42±0.21	0.43±0.15
	III	2.96±1.56[e]	2.44±0.46[f]	1.94±0.50	2.40±0.45	1.99±1.48	2.21±0.69
Buckwheat	I	1.50±0.51[e]	1.28±0.24[f]	1.18±0.35	1.32±0.25	0.97±0.44	1.03±0.28
	II	0.69[m]	0.59±0.36	0.59±0.41	0.60±0.35	0.48±0.45	0.50±0.37
	III	0.90±0.44	0.99±0.32	1.17±0.53	1.20±0.43	1.15±0.75	1.04±0.53

TABLE IV

COEFFICIENTS OF CORRELATION[a] BETWEEN THE OBSERVED AND THE COMPUTED DEMANDS FOR SELECTED COMMODITIES, CLASSIFIED ACCORDING TO THE PERIOD COVERED BY THE DATA AND THE TYPE OF DEMAND EQUATION ASSUMED
(Adjusted Data)

Commodity and period[b]		Type of demand equation assumed (See Table II)					
		(1)	(2)	(3)	(4)	(5)	(6)
Corn[c]	I	0.897	0.895	−0.887	−0.875	−0.908[d]	−0.908[d]
	II	0.942[e]	0.944[f]	−0.933	−0.927	−0.882	−0.934
	III	0.897	0.889	−0.773	−0.751	−0.853	−0.821
Hay[g]	I	−0.821[h]	−0.821[h]	−0.828[d]	−0.828[d]	−0.744	−0.750
	II	0.832[e]	0.821[f]	−0.779	−0.763	−0.753	−0.754
	III	0.922[e]	0.932[f]	−0.618	−0.608	−0.646	−0.710
Sugar	I	0.955	0.954	−0.581	−0.603	−0.358	−0.373
	II	0.976	0.974	−0.623	−0.628	−0.612	−0.613
	III	0.948[i]	0.960[j]	−0.706	−0.733	−0.836	−0.866
Potatoes	I	0.954[e]	0.947[f]	−0.907	−0.972	−0.898[k]	−0.970[k]
	II	0.952[l]	0.952[l]	−0.882	−0.900	−0.914[k]	−0.964[k]
	III	0.973	0.971	−0.964	−0.971	−0.918	−0.970
Oats[g]	I	0.9318[e]	0.9320[f]	−0.755	−0.749	−0.704	−0.781
	II	0.849	0.853	−0.821	−0.836	−0.822	−0.874
	III	0.708	0.703	−0.478	−0.469	−0.600	−0.595
Barley	I	0.801	0.803[f]	−0.271	−0.263	−0.518	−0.459
	II	0.907[e]	0.921[f]	−0.427	−0.419	−0.418	−0.411
	III	0.909[e]	0.902[f]	−0.587	−0.598	−0.568	−0.577
Rye	I	0.444	0.436	−0.443	−0.436	−0.410	−0.428
	II	0.911[e]	0.905[f]	−0.6044	−0.6038	−0.575	−0.569
	III	0.910[e]	0.926[f]	−0.881	−0.882	−0.798	−0.795
Buckwheat	I	0.912[e]	0.905[f]	−0.812	−0.807	−0.694	−0.702
	II	−0.596[m]	0.542	−0.390	−0.383	−0.305	−0.320
	III	0.836	0.860	−0.704	−0.707	−0.638	−0.629

FOOTNOTES TO TABLES III AND IV

[a] Simple correlation (r), multiple correlation (R), or index of correlation (ρ). Only the simple coefficients of correlation are preceded by their sign, which is negative.

[b] The periods are: I. 1875–1895; II. 1896–1914; III. 1915–1929, omitting 1917–1921. When link relatives are used, the number of observations is, of course, reduced by one except in the third period in which it is reduced by two on account of the omission of the war years.

While these periods have been adhered to for most commodities, a special study of the data from the point of view of comparability suggested the desirability of modifying the periods for some of the commodities. Thus the years 1875–1878 were omitted for corn and buckwheat; and the years 1893–1898 for corn, hay and oats when animal units were used. The reason for the latter omission is that the U.S.D.A. estimates for the number of horses and cattle in 1899 appear to have been raised in order to conform to the census figures for that year. In view of this change, the figures for 1893–1898 appear to be unusually low. Also, abnormal adjustments of crops to demand for oats in post-war years account for the extension of the war period omissions to 1923.

[c] Further analyses of the demand for corn were made by introducing an index of the number of animal units into the demand equation. The first section of the following table gives the results when consumption per animal unit is used. The second section gives the results when the number of animal units (z) is introduced into the equation as an explicit variable, together with total consumption (x), deflated price (y) and time (t).

Period	Correlation Coefficients		Elasticities of Demand (Negative Values)	
	Type of Equation		Type of Equation	
	$x=a+by+ct$	$x=Ay^{\alpha}e^{\beta t}$	$x=a+by+ct$	$x=Ay^{\alpha}e^{\beta t}$
I	0.8721	0.8719	0.69±0.20	0.74±0.13
II	0.919	0.910	0.61±0.13	0.66±0.084
III	0.882	0.880	0.51±0.20	0.52±0.15
	$x=a+by+cz+dt$	$x=Ay^{\alpha}z^{\beta}e^{\gamma t}$	$x=a+by+cz+dt$	$x=Ay^{\alpha}z^{\beta}e^{\gamma t}$
I	0.937	0.897	0.71±0.17	0.74±0.13
II	0.965	0.951	0.61±0.11	0.69±0.037
III	0.85581	0.85577	0.52±0.22	0.53±0.17

(For the third period, the equations are $x=a+by+ct+dt^2$, and $x=Ay^{\alpha}e^{\beta t+\gamma t^2}$, $x=a+by+cz+dt+et^2$, and $x=Ay^{\alpha}z^{\beta}e^{\gamma t+\delta t^2}$.)

[d] $(X+a)=AY^{\alpha}$. The standard error of η is not given since one of the parameters (a) was not obtained by the method of least squares.

[e] $x=a+by+ct+dt^2$.

[f] $x=Ay^{\alpha}e^{\beta t+\gamma t^2}$.

[g] In these commodities consumption is not per capita but per animal unit. When the number of animal units (z) is introduced into the equation as an explicit variable, together with total consumption (x), and deflated price (y), time (t) ceases to be a significant variable, and the resulting types of equations with their coefficients of correlation and elasticities of demand are as follows:

Commodity and Period		Correlation Coefficients		Elasticities of Demand (Negative Values)	
		Type of Equation		Type of Equation	
		$x=a+by+cz$	$x=Ay^{\alpha}z^{\beta}$	$x=a+by+cz$	$x=Ay^{\alpha}z^{\beta}$
Hay	I	0.948	0.957	0.74 ± 0.22	0.79 ± 0.16
	II	0.867	0.861	0.66 ± 0.20	0.70 ± 0.16
	III	0.776	0.838	0.52 ± 0.23	0.52 ± 0.16
Oats	I	0.963	0.979	0.52 ± 0.20	0.42 ± 0.12
	II	0.856	0.858	0.74 ± 0.24	0.75 ± 0.14
	III	0.570	0.596	0.53 ± 0.51	0.57 ± 0.42

(For oats, first period, the equations are $x=a+by+cz+dz^2$ and $x=Ay^{\alpha}z^{\beta}e^{\gamma(\log z)^2}$.)

[h] $(x+a)=Ay^{\alpha}$. The standard error of η is not given since one of the parameters (a) was not obtained by the method of least squares.

[i] $x=a+by+c(1-R^t)$.

[j] $x=Ay^{\alpha}e^{\beta(1-R^t)}$.

[k] For these periods, a slight improvement in the fit was obtained by fitting a curve of the type $X=AY^{\alpha}e^{\beta Y}$. The coefficients of multiple correlation are then 0.974 and 0.971, respectively. The corresponding elasticities of demand are -0.69 ± 0.43 and -0.62 ± 0.55.

[l] $x=A(y+a)^{\alpha}e^{\beta t+\gamma t^2}$.

[m] $x=a+b\log(y+a)$.

VI. CONCLUSIONS

Any explanation of these findings must be based on a thoroughgoing analysis of the soil in which they had lain embedded—the original data. But the presentation of this material with the detailed mathematical and graphical analysis to which it was subjected would require a large volume. Pending the appearance of the completed study, I must satisfy myself with the following observations:

1. It is clear from Table III that there is little or no difference between the results yielded by the straight line and by the hyperbolic demand curves, whether we use the time-regression method, the trend-ratio method, or the link-relative method. Does this mean that when one wishes to determine the coefficient of elasticity of demand for a commodity, the choice of curve is immaterial? The answer is, no. The various curves do not fit the data with the same degree of probability as can be seen from a comparison of the coefficients of correlation. That coefficient is generally to be preferred which is deduced from the demand curve that fits the data with the highest degree of probability.[34] Measured by the size of the coefficient of correlation between

[34] This criterion is based on the assumption that the dependent variable is always the same. It does not dispose of the prior question whether we should compare regressions of x on y, y on x, or mutual regressions; for the selection of

the observed and the computed values of the dependent variable, the curves which give the best fit generally belong to type (1) or to type (2), although there are important exceptions. These curves also have another advantage in that they express the direct relations between the quantities (consumption, price, etc.) with which we are primarily concerned, rather than between the trend ratios or the link relatives of these quantities. They afford, therefore, the best measure of the degree of elasticity of demand.

2. The question still remains, however, "Why do the different types of curves yield substantially the same degree of elasticity of demand?" Reference to Figure 3 will throw light on this question. That diagram shows clearly that *within the range of the observations* there is little difference between the fit of the logarithmic and the arithmetic surfaces. The difference between the surfaces increases as they are extrapolated beyond the range of the observations, but I have shown elsewhere[35] that the farther a curve is extrapolated, the larger its standard error. Beyond a certain point, all extrapolation becomes meaningless. In short, the comparatively small fluctuations of the observed data of prices and quantities fix comparatively narrow limits within which the shape of the curve can be determined with any degree of probability. Within these limits, there is no very significant difference between the fits of several types of curves. It is for this reason that these different curves yield coefficients of elasticity which are not significantly different from one another.

3. Though the time-regression method is generally the best method for dealing with the problem of demand, it can not always be relied upon to give the best results. As judged by the magnitude of the coefficient of correlation, the method of link relatives not infrequently gives better results than does the time-regression method. (See the correlations relating to corn and potatoes, first period.) The careful statistician will not, therefore, pin his faith on any one method.

4. Finally, I wish to call attention to the fact that the results summarized in Table III are all derived from market data of prices and quantities. It is quite possible that the corresponding results derived from family budget data would show interesting and important differences. But such results are not yet available. It is greatly to be desired

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one of these regressions involves important assumptions regarding the weights or errors of the different variables. (See my *Statistical Laws of Demand and Supply*, pp. 35–54.) A change from one type of regression to the next generally involves a much greater change in the elasticity of demand than does a change from one type of curve to the next, which does not involve a change in the independent variable.

[35] See my "Standard Error of a Forecast from a Curve," *loc. cit.*

that the advantages and the limitations of the various methods of deriving elasticities of demand and indexes of the marginal utility of money should be thoroughly worked out. It would be a fine task for an adequately equipped research organization to make such a comparative study. It need not fear that its results would be without practical application, for they are likely to throw a flood of light on business cycle phenomena and problems of taxation; to lead to a more realistic and useful type of economic theory, and to teach us how to wring fresh knowledge out of the accumulated masses of market and family budget data.

The University of Chicago

APPENDIX—BASIC DATA

TABLE 1.—INDEXES OR DEFLATORS USED, 1875–1929

Year	Population[a] of U. S. Jan. 1 of the follow'g yr. (Millions)	B.L.S. Index of Wholesale Prices (1913 = 100)[b]			Animal units[c] Jan. 1 of following year (Millions)		
		Dec.[d]	Ave. for yr. beginning				
			Aug.[e]	Jan.[f]	Hay	Oats[g]	Corn
1875	45.034	94[h]	106.8	113	12.004	11.432	16.572
1876	46.193	95[h]	101.8	103	12.354	11.693	17.636
1877	47.353	86[h]	87.3	99	12.724	12.043	19.321
1878	48.513	75[h]	77.5	82	13.557	12.741	20.921
1879	49.673	99	94.4	82	13.744	13.002	21.079
1880	50.902	97	97.8	98	13.938	13.225	21.586
1881	52.181	109	110.3	103	13.969	12.611	23.390
1882	53.460	102	105.4	111	14.994	13.072	23.655
1883	54.740	96	95.6	100	15.456	13.460	23.953
1884	56.019	84	85.8	93	15.942	13.916	24.294
1885	57.298	84	81.6	82	16.549	14.497	24.740
1886	58.577	80	80.2	80	17.202	14.994	24.640
1887	59.856	88	84.8	81	17.812	15.701	24.815
1888	61.136	86	84.0	87	18.340	16.246	26.331
1889	62.415	78	80.1	80	19.140	16.898	26.959
1890	63.709	81	80.5	82	19.077	16.741	26.937
1891	65.013	78	77.1	79	20.041	18.067	28.507
1892	66.318	77	76.7	76	20.161	18.662	27.484
1893	67.623	73	72.2	76	20.225	18.591	27.576
1894	68.928	69	68.8	70	19.819	18.367	27.217
1895	70.232	69	67.6	69	18.965	17.565	26.395
1896	71.537	66	65.4	66	18.274	16.798	25.456
1897	72.842	67	66.8	66	17.854	16.387	25.097
1898	74.146	69	70.5	67	17.563	16.072	24.764
1899	75.451	80	80.7	75	19.428	16.228	26.164
1900	76.938	81	80.0	82	21.912[i]	19.797[i]	28.673[i]
1901	78.556	82	82.2	80	21.955	19.541	27.419
1902	80.174	85	84.6	84	22.270	19.608	27.710
1903	81.792	83	83.8	84	22.493	19.825	28.336
1904	83.410	84	84.0	84	22.730	20.229	29.057
1905	85.028	89	88.8	86	24.156	22.296	30.705
1906	86.646	94	94.0	91	25.065	23.643	31.997
1907	88.264	93	93.2	96	25.044	23.889	32.777
1908	89.882	91	91.5	91	25.411	24.634	32.245
1909	91.500	98	97.0	94	25.145	24.908	30.589
1910	92.975	96	95.7	97	24.680	24.402	31.478
1911	94.390	96	97.5	95	24.616	24.601	31.467
1912	95.805	100	99.8	99	24.721	24.670	31.248
1913	97.220	99	98.8	100	25.403	25.150	31.392
1914	98.635	97	99.0	98	26.182	25.504	33.036
1915	100.050	108	112.5	101	26.956	25.728	34.121
1916	101.465	149	157.3	127	27.665	26.022	34.035
1917	102.880	182	187.3	177	28.240	26.524	35.406
1918	104.296	202	201.2	195	28.148	26.526	35.829
1919	105.711	223	229.3	207	26.930	25.345	34.300
1920	107.375	179	175.3	227	26.398	24.786	33.529
1921	109.040	140	143.6	147	26.248	24.372	33.499
1922	110.705	156	155.4	149	25.889	23.891	35.018
1923	112.370	151	150.0	154	25.369	23.273	33.956
1924	114.035	157	155.9	150	24.710	22.586	31.134
1925	115.700	156	155.1	159	23.934	21.956[j]	29.783
1926	117.364	147	146.6	151	23.144	21.193	29.626
1927	119.029	149	147.3	144[k]	22.608	20.489	30.223
1928	120.694	149	148.1	148[k]	22.365	19.887	29.331
1929	122.359	145	140.2	146[k]	22.414	19.416	28.434

NOTES TO TABLE 1

[a] The data for 1875–1919 were supplied by Clarence E. Batscheldt, Geographer, Department of Commerce. The data for 1920–1929 were computed by straight line interpolation from the population figures for July 1, supplied by Mr. Batscheldt.

[b] Taken from Carl Snyder, *Business Cycles and Business Measurements*, New York: The Macmillan Company, 1927, p. 288. "The sources for . . . this index are as follows: Commodity Prices at Wholesale: 1875–1889, Monthly Index of Basic Commodities compiled by Federal Reserve Bank of New York; 1890–1899, monthly figures interpolated from quarterly series of the United States Department of Labor; 1900 to date, Department of Labor monthly index." (*Ibid.*, p. 137.) For the years following 1926, the index was taken from the Bureau of Labor Statistics Bulletins. In 1928, the Bureau of Labor Statistics made several modifications in its index numbers of wholesale prices and changed the base to 1926 = 100. The new index numbers for 1928 and 1929 were, therefore, spliced on to the old series as follows: The new index (1926 = 100) was first shifted to 1913 as a base by dividing by the 1913 average. Then the figures for 1928 and 1929 thus obtained were multiplied by the ratio for 1927 of the old index to the new index with 1913 as a base.

[c] The animal unit series for hay and oats is a weighted index of the number of milk cows, of other than milk cows, and of horses and mules for each year, the weights being the consumption of the commodity by each of these groups in 1914. The animal unit series for corn is a weighted index of the number of hogs, of milk cows, of other than milk cows, and of horses and mules. (See U.S.D.A. Farmers' Bulletin, No. 629, p. 9.)

[d] The December index was used to deflate the price series of the following commodities: Corn, Hay, Wheat, Potatoes, Oats, Rye, and Buckwheat.

[e] The index for the year beginning August was used to deflate the price of barley, which is a Chicago price for the year beginning August.

[f] The index for the year beginning January was used to deflate the price of sugar, which is a New York price for the year beginning January.

[g] In the actual computations, one more significant figure was used.

[h] Reduced to a gold base, since the farm prices as given in the Yearbooks of Agriculture are on a gold base. The method followed was to divide the December index on a currency base by the average price of gold in greenbacks for December. (See W. C. Mitchell, *Gold, Prices, and Wages*, Berkeley, California: University Press, 1908, pp. 4–13.)

[i] After the computations were completed, the U.S.D.A. revised the figures for milk cows and other than milk cows for 1900–1929 and for horses and mules for 1919–1929. (See Yearbook of Agriculture, 1932, p. 770, Table 313; p. 818, Table 378; and p. 825, Table 386.) A comparison shows, however, that the revisions would not materially affect our conclusions.

[j] The incorrect figure, 21.950, was used in the computations.

[k] Through an oversight, a slight inconsistency crept into the method of splicing the index used to deflate sugar prices. The figures for 1927–1929 (1913 = 100) were multiplied by the ratio for 1926, instead of for 1927, of the old index to the new index with 1913 as a base. (See note b.)

TABLE 2—QUANTITIES AND PRICES USED,[a] 1875–1929

Year[b]	Corn		Hay		Wheat	
	10,000 bu.[c]	Cents per bu.[d]	1,000 short tons[c]	Dollars per ton[d]	10,000,000 bu.[c]	Cents per bu.[d]
1875	132.107	36.7	27,874	10.78		
1876	128.383	34.0	30,867	8.97		
1877	134.256	34.8	31,629	8.37		
1878	138.822	31.7	39,608	7.20		
1879	154.790[i]	37.5[i]	39,862	9.31		
1880	171.744	39.6	31,925	11.65		
1881	119.492	63.6	35,135	11.82		
1882	161.702	48.5	38,138	9.73		
1883	155.107	42.4	46,864	8.19		
1884	179.553	35.7	48,470	8.17		
1885	193.618	32.8	44,732	8.71		
1886	166.544	36.6	41.796	8.46		
1887	145.616	44.4	41,454	9.97		
1888	198.779	34.1	46,643	8.76		
1889	199.865	27.4	49,181	7.76		
1890	146.041	50.0	49,057	8.18		
1891	205.582	39.7	48,759	8.89		
1892	171.369	38.8	49,238	8.95		
1893	170.757	35.9	55,575	9.48		
1894	133.968	45.1	50,468	8.96		
1895	231.095	25.0	41,838	9.46		
1896	250.348	21.3	54,380	7.48	40.4630	71.7
1897	214.455	26.0	58,878	7.28	34.4331	80.9
1898	226.112	28.4	66,772	6.63	37.5827	58.2
1899	245.463[k]	29.9	57,450	8.20	37.0281	58.6
1900	250.515	35.1	53,231	9.72	36.2226	62.0
1901	160.729[k]	60.0[k]	55,819	9.91	47.3880	62.6
1902	262.070[k]	40.0[k]	65,296	9.19	46.8987	63.0
1903	233.942[k]	42.1	68,154	9.35	47.6698	69.5
1904	252.068[k]	43.7	69,192	8.91	50.1104	92.4
1905	274.433[k]	40.7[k]	72,973	8.59	51.2809	74.6
1906	289.582[k]	39.2[k]	66,341	10.43	51.0346	66.2
1907	251.206	50.9	72,261	11.78	47.3951	86.5
1908	254.496	60.0	78,440	9.14	49.1054	92.2
1909	257.234	58.6	74,384	10.58	49.3995	98.4
1910	288.626	48.0	69,378	12.14	48.5008	88.3
1911	253.149	61.8	54,916	14.29	48.4342	87.4
1912	312.475	48.7	72,691	11.79	50.2329	76.0
1913	244.699	69.1	64,116	12.43	55.4367	79.9
1914	267.280	64.4	70,071	11.12	49.7179	98.6
1915	299.479	57.5	85,920	10.63	59.0789	91.9
1916	256.693	88.9	91,192	11.22	49.8026	160.3
1917	306.523	127.9	83,308	17.09	47.0472	200.8
1918	250.266	136.5	76,660	20.13	51.3525	204.2
1919	281.130	134.5	86,997[k]	20.05[k]	59.6163	214.9
1920	320.858	67.0	89,785[k]	17.66[k]	44.9863	143.7
1921	306.857	42.3	82,458[k]	12.10[k]	46.5114	92.6
1922	290.602	65.8	95,748[k]	12.55[k]	54.7668	100.7
1923	305.356	72.6	89,250[k]	14.13	58.3731	92.3
1924	230.941	98.2	97,224[k]	13.76[k]	54.6199	129.9
1925	291.611	67.4	85,431[k]	13.93[k]	51.2201	141.6
1926	269.153	64.2	86,144[k]	14.10[k]	50.9724	119.8
1927	276.309	72.3	106,001[k]	11.35[k]	58.9579[k]	111.5
1928	281.890	75.2	93,351[k]	12.27[k]	56.2098[k]	97.0
1929	261.413[k]	78.1[k]	100,893[k]	12.22[k]	54.7752[k]	104.2[k]

TABLE 2, *continued*

Year[b]	Sugar		Potatoes		Oats	
	1,000 tons[f]	Cents per lb.[g]	1,000,000 bu.[c]	Cents per bu.[d]	10,000,000 bu.[c]	Cents per bu.[d]
1875	886.6	10.718	166.877	34.4	35.4318	32.0
1876	868.6	10.468	124.827	61.9	32.0884	32.4
1877	834.7	11.312	170.092	43.7	40.6394	28.4
1878	866.3	9.484	124.127	58.7	41.3579	24.6
1879	931.7	8.785	181.626	43.6	45.0745	33.3
1880	1072	9.602	167.660	48.3	41.7885	36.0
1881	1134	9.667	109.145	91.0	41.6481	46.4
1882	1271	9.234	170.973	55.7	48.8251	37.5
1883	1371	8.506	208.164	42.2	57.1302	32.7
1884	1467	6.780	190.642	39.6	58.3628	27.7
1885	1454	6.441	175.029	44.7	62.9409	28.5
1886	1634	6.117	168.051	46.7	62.4134	29.8
1887	1548	6.013	134.103	68.2	65.9618	30.4
1888	1702	7.007	202.365	40.2	70.1735	27.8
1889	1586	7.640	201.200	35.4	80.1586	21.9
1890	1654	6.171	150.494	75.3	57.2671	41.7
1891	2116	4.641	256.122	35.6	83.9995	30.6
1892	2076	4.346	164.516	65.5	69.5277	31.5
1893	2136	4.842	195.040	58.4	67.6151	29.1
1894	2254	4.120	183.841	52.9[j]	71.5535	32.1
1895	2184	4.152	317.114	26.2	88.5959	19.4
1896	2195	4.532	271.769	29.0	78.0124	18.3
1897	2319	4.503	191.025	54.2	79.1442	20.8
1898	2243	4.965	218.772	41.5	84.2747	25.2
1899	2327	4.919	260.257	39.7	92.5555	24.5
1900	2486	5.320	247.759	42.3	91.3800	25.4
1901	2657	5.050	198.626	76.3	77.8392	39.7
1902	2874	4.455	293.918	46.9	105.3489	30.6
1903	2856	4.638	262.053	60.9	86.9350	34.0
1904	3099	4.772	352.268	44.8	100.8931	31.1
1905	2948	5.256	278.885	61.1	109.0236	28.9
1906	3208	4.515	331.685	50.6	103.5576	31.9
1907	3353	4.649	322.954	61.3	80.5108	44.5
1908	3568	4.957	302.000	69.7	85.0540	47.3
1909	3649	4.765	394.553	54.2	106.8289	40.6
1910	3752	4.972	349.032	55.7	118.6341	34.4
1911	3754	5.345	292.737	79.9	92.2298	45.0
1912	3925	5.041	420.647	50.5	141.8337	31.9
1913	4192	4.278	331.525	68.7	112.1768	39.2
1914	4212	4.683	409.921	48.7	114.1060	43.8
1915	4258	5.559	359.721	61.7	154.903	36.1
1916	4098	6.862	286.953	146.1	125.184	52.4
1917	4126	7.663	442.108	122.8	159.274	66.6
1918	3915	7.834	411.860	119.3	153.812	70.9
1919	4556	9.003	322.867[k]	159.5[k]	118.403[k]	70.4[k]
1920	4575	11.390	403.296[k]	114.5[k]	149.628[k]	46.0[k]
1921	4600	6.207	361.659[k]	110.1[k]	107.834[k]	30.2[k]
1922	5704	5.904	453.396[k]	58.1[k]	121.580[k]	39.4[k]
1923	5354	8.441	416.105[k]	78.1[k]	130.588[k]	41.4[k]
1924	5437	7.471	419.560[k]	62.5[k]	150.253[k]	47.7[k]
1925	6171	5.483	320.915[k]	187.0[k]	148.755[k]	38.0[k]
1926	6352	5.473	354.458[k]	141.4[k]	124.685[k]	39.8[k]
1927	5933	5.828	402.741[k]	96.5[k]	118.259[k]	45.0[k]
1928	6208	5.540	465.350[k]	53.9[k]	143.941[k]	40.9[k]
1929	6508	5.025	359.048[k]	130.9[k]	122.837[k]	43.5[k]

TABLE 2, *continued*

Year[b]	Barley		Rye		Buckwheat	
	1,000,000 bu.[f]	Cents per bu.[h]	1,000,000 bu.[f]	Cents per bu.[d]	1,000,000 bu.[e]	Cents per bu.[d]
1875	47.117	51	17.083	67.1	10.082	62.0
1876	44.462	39	18.141	61.4	9.669	66.6
1877	38.212	37	17.011	57.6	10.177	66.9
1878	47.644	40	20.896	52.5	12.247	52.6
1879	55.636	50	22.236	67.6	17.530	60.3
1880	54.424	74	22.599	75.6	14.618	59.4
1881	54.160	86	19.798	93.3	9.486	86.5
1882	59.772	54	27.738	61.5	11.019	73.0
1883	58.113	47	21.721	58.1	7.669	82.2
1884	70.861	42	25.620	51.9	11.116	58.9
1885	68.535	42	21.629	57.9	12.626	55.9
1886	68.584	45	23.995	53.8	11.869	54.5
1887	67.204	63	20.590	54.5	10.844	56.5
1888	73.878	53	28.106	58.8	12.050	63.3
1889	88.248	42	26.137	42.3	12.109	50.5
1890	76.842	66	26.074	62.6	12.678	57.3
1891	96.342	50	20.756	77.1	13.013	57.0
1892	90.837	50	27.768	53.6	12.643	52.0
1893	79.164	48	28.343	50.2	12.866	58.3
1894	78.456	51	29.594	49.4	13.721	55.7
1895	107.696	31	30.128	42.2	16.748	45.3
1896	80.373	30	20.339	38.8	15.805	39.3
1897	91.801	40	17.904	43.2	17.260	42.1
1898	97.356	45	22.719	44.5	13.961	45.0
1899	92.793	43	27.952	49.6	13.001	55.9
1900	89.596	56	28.446	49.8	11.810	55.8
1901	112.765	64	28.391	55.4	15.693	56.4
1902	140.703	56	29.811	50.5	15.286	59.6
1903	135.677	56	31.239	54.0	15.248	60.8
1904	151.084	49	31.796	68.9	16.327	62.5
1905	151.679	50	33.781	60.4	15.797	58.6
1906	183.638	61	35.790	58.5	15.734	59.7
1907	165.638	84	33.012	72.5	14.858	70.0
1908	178.132	67	34.473	72.8	16.541	75.7
1909	183.524	67	35.194	72.2	17.983	70.2
1910	164.512	92	35.084	71.5	17.598	66.1
1911	161.357	122	33.222	83.2	17.549	72.6
1912	205.965	68	33.810	66.3	19.249	66.1
1913	171.595	65	39.145	63.4	13.833	75.5
1914	166.344	72	29.899	86.5	16.881	76.4
1915	198.068	69	39.366	83.4	15.056	78.7
1916	162.452	119[l]	35.587	122.1	11.662	112.7
1917	183.559	146	46.581	166.0	16.022	160.0
1918	226.924	104	55.212	151.6	16.905	166.5
1919	113.252[k]	145	35.029[k]	133.2[k]	14.399[k]	146.1[k]
1920	162.098[k]	78	13.605[k]	126.8[k]	13.142[k]	128.3[k]
1921	127.408[k]	61	32.431[k]	69.7[k]	14.207[k]	81.2[k]
1922	160.197[k]	65	51.798[k]	68.5[k]	14.564[k]	88.5[k]
1923	183.833[k]	72	43.177[k]	65.0[k]	13.965[k]	93.3[k]
1924	153.080[k]	90	15.279[k]	106.4[k]	13.357[k]	102.6[k]
1925	183.468[k]	72	33.810[k]	78.2[k]	13.994[k]	88.8[k]
1926	165.300[k]	77	19.052[k]	83.4[k]	12.676[k]	88.2[k]
1927	226.652[k]	91	31.819[k]	85.3[k]	15.755[k]	83.5[k]
1928	297.238[k]	60	33.879[k]	86.0[k]	13.148[k]	87.5[k]
1929	278.879[k]	62	39.312[k]	86.4[k]	11.474[k]	97.7[k]

NOTES TO TABLE 2

[a] The data for all commodities except wheat and sugar are taken from the Yearbooks of Agriculture for 1920, 1921, 1927 and 1931. The data for wheat are taken from U.S.D.A. Statistical Bulletin No. 12, as well as the Yearbooks of Agriculture, 1926–1931. The data for sugar are taken from Palmer's *Concerning Sugar*, a loose-leaf service of the United States Sugar Manufacturers' Association.

[b] The year begins July 1 for all commodities except sugar, for which the year begins January 1.

[c] The U.S.D.A. estimates for production have been used. (See text.)

[d] December 1 farm price. (See text.)

[e] This is a total disappearance series, which is production plus imports and beginning stocks minus exports, end stocks and seed requirements. Data for the first period are not available.

[f] These figures represent "consumption," i.e., production minus net exports. The consumption series for barley and rye are based on U.S.D.A. data. The consumption series for sugar is given by the United States Sugar Manufacturers' Association.

[g] Average New York wholesale price of refined sugar for year beginning January 1.

[h] An August–July average of quotations for choice to fancy or fair to good malting grades in Chicago.

[i] Revisions of these figures may be found in the Yearbook of Agriculture, 1927, p. 774, Table 47.

[j] The incorrect figure, 52.8, was used in our computations.

[k] Revisions of these figures may be found in the Yearbook of Agriculture, 1932.

[l] This is the correct figure and is taken from the Yearbook of Agriculture for 1927. In recent yearbooks it is given as 191 through a typographical error. (Letter of July 7, 1931, from Dr. O. C. Stine of the Division of Statistical and Historical Research.)

CAN STOCK MARKET FORECASTERS FORECAST?

By Alfred Cowles 3rd

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INTRODUCTION

This paper presents results of analyses of the forecasting efforts of 45 professional agencies which have attempted, either to select specific common stocks which should prove superior in investment merit to the general run of equities, or to predict the future movements of the stock market itself. The paper falls into two main parts. The first deals with the attempts of two groups, 20 fire insurance companies and 16 financial services, to foretell which specific securities would prove most profitable. The second part deals with the efforts of 25 financial publications to foretell the future course of the stock market. Various statistical tests of these results are given.

These investigations were instituted five years ago as a means of testing the success of applied economics in the investment field. It seemed a plausible assumption that if we could demonstrate the existence in individuals or organizations of the ability to foretell the elusive fluctuations, either of particular stocks, or of stocks in general, this might lead to the identification of economic theories or statistical practices whose soundness had been established by successful prediction.

The forecasters include well-known organizations in the different fields represented, many of which are large and well financed, employing economists and statisticians of unquestioned ability. The names of these organizations are omitted, since their publication would be likely to invite wholesale controversy over the interpretation of their records. Some of the forecasters seem to have taken a page from the book of the Delphic Oracle, expressing their prophecies in terms susceptible of more than one construction. It would frequently be possible, therefore, for an editor, after the event, to present a plausible challenge of our interpretation. Most of the forecasts appear through the medium of weekly publications and each of these has been read and recorded on the day it became available to us, which in practically every case was before the event. In this way certain possible elements of bias have been eliminated. It was impossible that hindsight could influence our judgment, either in the selection of publications for analysis or in the interpretations placed on their forecasts. In the case of the fire insurance companies, however, the analyses were made annually, based on the transactions reported in *Kimber's Record of Insurance Company Security Purchases*. The companies were selected as fairly representa-

tive of their class. The analysis of the 26-year forecasting record of William Peter Hamilton, former editor of the *Wall Street Journal*, also falls in a different category, in that it was undertaken because of the reputation for successful forecasting which he had established over a long period of years.

FORECASTING THE COURSE OF INDIVIDUAL STOCK PRICES

We turn first to the records of two groups, the financial services and the fire insurance companies, which have attempted to select individual stocks that would prove more profitable for investment than the average issue. The first part of this section deals with the records, over the 4½ years ending July, 1932, of 16 leading financial services which have made a practice of regularly submitting to their subscribers selected lists of common stocks for investment. Our analysis includes about 7,500 separate recommendations, requiring approximately 75,000 entries. The first step was to record each week the name and price of each stock recommended for purchase or sale by each service. Next came the tabulation of the advice to sell or cover the commitment previously advised. Reiterated advice was not considered, action being assumed to have been taken as of the date when the recommendation was first published. The percentage gain or loss on each such transaction was recorded and, in a parallel column, the gain or loss of the stock market for the identical period. A balance was struck every six months which summarized the total results secured by each service as compared with the action of the stock market. Proper corrections were, of course, made to offset the effect of changes in capital structure resulting from the issue of rights, stock dividends, etc. Since a tendency existed among some services to emphasize their conspicuously successful stock recommendations and ignore more unfortunate commitments, we adopted a practice of automatically dropping a stock from the list six months after it had been last recommended, when specific advice to sell was not given.

A redistribution of funds in equal amounts among all stocks recommended has been assumed for each service at the beginning of every six months' period analyzed. It could be maintained, of course, that this equalizing process should take place as often as once a week, but this would increase the labor of computation to overwhelming proportions. Provisional experiments demonstrated that it would yield conclusions practically identical with those secured by the shorter method. Compounding the successive six months' records gives the percentage by which each service's recommendations have exceeded, or fallen behind, the stock market, as shown in Table I.

Only six of the 16 services achieved any success. To arrive at an

average performance, the record of each service was reduced to an effective annual rate which was then weighted in accordance with the length of the period represented. The average annual effective rate of all the services, thus arrived at, is −1.43 per cent.

TABLE I

RESULTS OF COMMITMENTS IN STOCKS RECOMMENDED BY 16 FINANCIAL SERVICES (RELATED TO MARKET AVERAGES)

Service	*Weeks*	*Per cent*
1.	234	+20.8
2.	234	+17.2
3.	234	+15.2
4.	234	+12.3
5.	234	+ 8.4
6.	26	+ 6.1
7.	52	0.
8.	104	− 0.5
9.	234	− 1.9
10.	52	− 2.2
11.	52	− 3.0
12.	52	− 8.3
13.	78	−16.1
14.	104	−28.2
15.	104	−31.2
16.	156	−33.0

PROBABILITY TESTS

In an attempt to determine whether the service having the best record achieved its result through skill or chance, we resorted to the theories of compound and inverse probability. Our conclusion is thus rendered consistent by obtaining approximately the same answer in two different ways.

With the aid of various checks, involving 1250 computations of the action of individual stocks selected at random, we derived a formula, A.D. $(t) = 5.42 + 1.5t$ (A.D. = average deviation, t, in units of 4 weeks, $\geqq 1$), representing the deviation, for all periods from one month up to one year, of the average individual stock from the average of all stocks.

Service Number 1, for the 9 six months' periods from January 1, 1928 to July 1, 1932, was successful 7 times and unsuccessful 2 times. With the aid of the table referred to, the averages of "chances in 1000 to do worse" for the 7 periods in which it was successful and the 2 periods in which it was unsuccessful were found to be 842 and 66 respectively. By the theory of direct probabilities, the probability of a single service being right at least 7 times in 9 is equal to the sum of the first 3 terms of the binomial $(\frac{1}{2}+\frac{1}{2})^9$.

$$p = 1/2^9 + 9/2^9 + 36/2^9 = 46/512 = .090$$

The probability that a single service could in 9 predictions be 7 times

on the positive side and in these 7 forecasts equal the achievement of Service Number 1 is,

$$P = .090 \times (1 - .842) = .014$$

However, the record of the best service is marred by its failure in the two negative cases. The average of the two chances to do worse in these cases is .066. We then have,

$$Q = (7/9) \times .842 + 2/9 \times .066 = .670$$

as the probability of a single random service having a record worse than that of Service Number 1. We therefore conclude that the probability that a random service can, first, be on the right side of the market 7 times out of 9, and second, equal in performance the record of Service Number 1, is

$$P = .090 \times (1 - .670) = .030.$$

This means that in 16 services we should expect to find $16 \times .030 = .48$ services which will equal the record of Service Number 1. That is to say, the chance is even that we should get at least one service as good as Number 1.

Because of the assumptions implied in this computation, we shall argue this another way. We shall assume that the probability that a service for its total forecast shall be on the positive side of the market is 1/2. Then the estimate of its success must be made by a different evaluation of Q. For this purpose we shall adopt a formula suggested by Bayes' rule in inverse probability in which the weights .910 and .090 instead of 7/9 and 2/9 are used. We get

$$Q = \frac{.910\ (.842)}{.910\ (.842) + (.090)\ (.934)} = .901$$

Hence, if a service was on the right side of the market, the probability of its achieving the success of Service Number 1 would be $1-Q$. Thus the compound probability would be

$$P = 1/2\ (1 - .901) = .050.$$

Among the 16 services the probability of the most successful one equalling the record of Service Number 1 would be $P = 16 \times .050 = .80$, that is to say, we should expect to get among 16 random services about one service which would equal Number 1. Since this answer is quite consistent with our previous answer, our analysis suggests the conclusion that the record of Service Number 1 could not be definitely attributed to skill.

TWENTY FIRE INSURANCE COMPANIES

The second analysis deals with the common stock investments, from 1928 to 1931 inclusive, of 20 of our leading fire insurance companies. Its significance lies in the fact that these companies are representative of a class of common stock investor which has had long years of experience and large amounts of capital at its disposal. Fire insurance dates from the Great London Fire of 1666, and active investment in stocks developed during the nineteenth century. The fire insurance companies are much older hands at the business of investment than either the financial services, which are a twentieth century product, or American investment trusts, which are largely a development of the last few years. The investment policies of these companies are based on the accumulated knowledge of successive boards of directors whose judgment might be presumed, over the years, to have been well above that of the average investor. The 20 companies which were selected for analysis hold assets totalling several hundred million dollars, and seem a fair sample of their kind.

Fire insurance companies carry between 20 and 30 per cent of their total investments in common stocks. Their average turnover amounts to only some 5 per cent a year. For this reason it was thought best to confine our analysis to the record of the actual purchases and sales made during the period under examination, rather than to compute the record of the entire common stock portfolio. To simplify the labor, all items of stock purchased were given equal weights, regardless of the amounts involved. While the conclusion does not exactly reflect the actual investment results secured by these companies, it should, however, provide a satisfactory test of the success of these organizations in selecting stocks which performed better than the average.

The method employed in the analysis is essentially the same as that used in the case of the investment services. A second purchase of an item was omitted from the record unless a sale of this item intervened. A record of the sale of an item, of course, determined the date as of which it was dropped from the list. Also, any item of which there had been no purchase recorded for 12 months was automatically considered sold.

The compounded records of the 20 companies for the 4-year period are shown in Table II.

Six of the companies show evidence of success, and the average of the 20 is −4.72 per cent. The average record of the companies in the stocks which they selected for investment fell below the average of the stock market at the effective annual rate of 1.20 per cent. A comparable result could have been achieved through a purely random selection of

stocks. The analysis of the fire insurance companies' records thus confirms the results secured in appraising the records of the financial services.

TABLE II

RESULTS OF COMMITMENTS IN STOCKS MADE BY TWENTY FIRE INSURANCE COMPANIES (RELATED TO STOCK MARKET AVERAGES)

All companies 1928–31, inc.

Company	*Per cent*
1.	+27.35
2.	+25.11
3.	+18.34
4.	+10.38
5.	+10.12
6.	+ 3.20
7.	− 2.06
8.	− 3.63
9.	− 5.06
10.	− 6.67
11.	−10.44
12.	−10.55
13.	−11.76
14.	−12.92
15.	−13.82
16.	−14.96
17.	−18.03
18.	−21.89
19.	−23.44
20.	−33.72

FORECASTING THE STOCK MARKET ACCORDING TO THE DOW THEORY

Having dealt with the efficiency of two great groups of professionals, fire insurance companies and financial services, in selecting common stocks for investment, we turn now to a consideration of skill in predicting the course of the stock market as a whole. This section also is in two principal sub-divisions. First we consider the record of William Peter Hamilton.

This analysis was undertaken because several decades of editorials in the country's leading financial newspaper have built up a great popular following for the Dow Theory, of which Hamilton was the principal sponsor. The Dow Theory was the creation of Charles H. Dow, founder of the Dow Jones financial news service, founder and editor of the *Wall Street Journal*. After Dow's death in 1902 Hamilton succeeded him as editor of the *Wall Street Journal*, continuing in this position until his death in December, 1929.

During 26 years of his incumbency Hamilton wrote 255 editorials which presented forecasts for the stock market based on the Dow Theory. These were sufficiently definite to permit scoring as bullish, bearish, or doubtful. This we did by a majority vote of five readers. When

doubtful we assumed that he abstained from trading. When bullish it was assumed that he bought equal dollar amounts of the stocks included in the Dow Jones railroad and industrial averages, and sold them only when he became bearish or doubtful. When bearish we assumed that he sold short equal dollar amounts of these stocks and covered only when he became doubtful or bullish. The percentage gain or loss on each such transaction has been calculated, and the results compounded through the 26 years. Since the Dow Jones averages have only recently been corrected to offset the effect of stock rights, stock dividends, and stock splits, such adjustments have been made for all the previous years on the basis of tables published by Dwight C. Rose in his book *Investment Management*. Corrections have also been made to allow for the effect of brokerage charges, cash dividends, and the interest presumably earned by Hamilton's funds when they were not in the stock market. The fully adjusted figures were then reduced to an effective annual rate of gain which is presented as a measure of the result accomplished.

From December 1903 to December 1929, Hamilton, through the application of his forecasts to the stocks composing the Dow Jones industrial averages, would have earned a return, including dividend and interest income, of 12 per cent per annum. In the same period the stocks composing the industrial averages showed a return of 15.5 per cent per annum. Hamilton therefore failed by an appreciable margin to gain as much through his forecasting as he would have made by a continuous outright investment in the stocks composing the industrial averages. He exceeded by a wide margin, however, a supposedly normal investment return of about 5 per cent. Applying his forecasts to the stocks composing the Dow Jones railroad averages, the result is an annual gain of 5.7 per cent while the railroad averages themselves show a return of 7.7 per cent.

Hamilton was long of stocks 55 per cent, short 16 per cent, and out of the market 29 per cent, of the 26 years under review. Counting only changes of position, he made bullish forecasts 29 times. Applying these to the industrial averages, 16 were profitable, 13 unprofitable. He announced bearish forecasts 23 times, 10 were profitable, 13 unprofitable. He advised 38 times that funds be withdrawn from the stock market, 19 of these withdrawals being profitable, 19 unprofitable. In all, 45 of his changes of position were unsuccessful, 45 successful. The application of the forecasts to the railroad averages confirms these conclusions except that in this case 41 changes of position were successful and 49 unsuccessful. For the period from 1909 to 1914 inclusive, when the industrial averages displayed what, in effect, was a horizontal trend, his hypothetical fund shrank 7.8 per cent per annum below what it would

have been if loaned at 5 per cent interest. The result of applying his forecasts to the railroad averages deserves attention in view of the fact that this group displayed an almost horizontal secular trend for the 26 years under consideration. His average annual gain of 5.7 per cent in this group would have been approximately equalled, in the case of a continuous outright investment, by the dividend income.

STOCK MARKET FORECASTS OF TWENTY-FOUR FINANCIAL PUBLICATIONS

For the analysis of other results secured in forecasting the course of the stock market, we selected during the period from January 1, 1928, to June 1, 1932, 24 publications (among which were 18 professional financial services, 4 financial weeklies, one bank letter, and one investment house letter). More than 3,300 forecasts were tabulated. The method used has been for each reader to ask himself the question, "In the light of what this particular bulletin says, would one be led to buy stocks with all the funds at his disposal, or place a portion only of his funds in stocks, or withdraw entirely from the market?" The reader graded the advice in each instance by means of one of nine possible entries, namely 100 per cent of funds in the market, $87\frac{1}{2}$, 75, $62\frac{1}{2}$, 50, $37\frac{1}{2}$, 25, $12\frac{1}{2}$, or 0 per cent. The great majority of forecasters confine themselves to general discussions of the investment situation, leaving to the reader the decision as to what proportion of his funds he shall place in the market. The tabulation, therefore, cannot be mathematically conclusive. Our method, in general, has been to have the vote of three readers of competent intelligence determine the interpretation of each forecast. Marginal commitments have not been incorporated in our tabulations because in no case have they been advised by any of the forecasters. Similarly, short commitments are not in general assumed because, of the entire 24 forecasters, only one recommended them. His record has been computed on a special basis.

The tabulated forecasts have been tested in the light of the actual fluctuations of the stock market as reflected by the Standard Statistics Company index of 90 representative stocks. If a forecast is 100 per cent bullish and the market rises 10 per cent in the subsequent week, the forecaster is scored as +10 per cent. If the forecaster, after weighing the favorable and unfavorable factors, leaves the decision hanging in the balance, the score is +5 per cent or one-half of the market advance. This is on the assumption that the investor, being in doubt as to the future course of the market and being, by definition, committed to common stocks as a possible investment medium, would be led to adopt a hedged position with half of his funds in stocks and half in reserve. If the forecast is 100 per cent bearish, the score is zero, regard-

less of the subsequent action of the market, on the assumption that, under such conditions, the investor would withdraw all of his funds from stocks. On the other hand, if the forecast is 100 per cent bullish and the market drops 10 per cent in the ensuing week, the score is −10 per cent. If the forecast is doubtful when the market drops 10 per cent, the score is −5 per cent. The compounding of all these weekly scores for the period covered gives a cumulative record for each forecaster. This permits comparisons which reveal relative success and average performance. While it may be thought that accurate week-to-week forecasting is a hopeless ideal, it should be emphasized that our analysis of weekly results also measures accurately the efficiency of long swing forecasts.

A figure representing the average of all possible forecasting results for the period was arrived at by compounding one-half of every weekly percentage change in the level of the stock market. The final scores given below for the 24 forecasters were computed by dividing the actual performance of each by the average of all possible results referred to above, and subtracting 100.

TABLE III
RESULTS OF STOCK MARKET FORECASTS

Forecaster	*Weeks*	*Per cent*
1.	105	+72.4
2.	230	+31.5
3.	230	+28.3
4.	21	+24.2
5.	157	+ 9.0
6.	53	+ 3.0
7.	126	+ 2.4
8.	53	+ 1.3
9.	105	− 1.7
10.	157	− 2.1
11.	230	− 3.6
12.	43	− 6.0
13.	53	− 6.7
14.	131	− 6.9
15.	230	−12.5
16.	230	−13.5
17.	53	−17.2
18.	230	−21.5
19.	69	−29.4
20.	230	−33.0
21.	230	−35.3
22.	230	−41.5
23.	157	−45.3
24.	230	−49.1

The records show that only one-third of the list met with any success. In order to derive a significant average of the performance of the

entire group the results listed above have been reduced to effective annual rates, and each has been given a weight to conform with the length of the record analysed. After these adjustments, we are enabled to conclude that the average forecasting agency fell approximately 4 per cent per annum below a record representing the average of all performances achievable by pure chance. This would seem to indicate that, in general, these stock market forecasters failed to accomplish their objective. The most that can be said in extenuation is that the long-continued decline in securities has been, naturally, a handicap to a group which, taking warning from the experience of Cassandra, usually seems constrained to look on the bright side. During the $4\frac{1}{2}$ year period under analysis the number of weeks in which the stock market declined almost exactly equalled the number of weeks in which advances were recorded, and the total amount of the declines considerably exceeded the total amount of the advances. Yet we recorded during this period 2035 bullish, 804 bearish, and 479 doubtful forecasts. Further, we note that in 1928, the only year the market showed a net gain, the excess of bullish over bearish forecasts was smaller than in any succeeding year. Taking a glaring example, in the rising market of 1928 the ratio of bullish to bearish forecasts was only four to three. In 1931, when the market declined 54 per cent, there were sixteen bullish forecasts to every three bearish.

STATISTICAL INTERPRETATIONS OF RESULTS

In an attempt to illuminate the problem of whether the records of all these forecasters lay within the limits of pure chance, we compiled 24 records, identical with those of the 24 forecasters as to the total period covered, but having purely fortuitous advices applied to random intervals within these perids. For example, to compile a purely chance record to compare with the actual record of a forecaster whose operations covered 230 weeks from January 1, 1928, to June 1, 1932, we first determined the average number of changes of advice for such a period, which was 33. Cards numbered from 1 to 229 were shuffled, drawn, reshuffled, drawn, in all 33 times. Thus 33 random dates were selected as of which forecasts were to be changed. The investment policies which were to apply to the intervals between those dates were derived in similar fortuitous fashion, by drawing 33 times from nine cards on each of which a different one of the nine possible investment policies was noted.

It only remained to relate these random advices to a stock market index, cumulate the results, relate them, as we had done with the records of the actual forecasters, to the average of all chances for the period, and subtract 100. Thus we had a list of 24 purely chance fore-

casting records, shown in Table IV, to compare with the records of the actual prophets.

TABLE IV

RESULTS OF RANDOM FORECASTS

Forecaster	*Weeks*	*Per cent*
1.	230	+71.1
2.	230	+37.2
3.	230	+24.2
4.	157	+19.1
5.	230	+13.2
6.	105	+ 9.2
7.	230	+ 2.7
8.	53	+ 2.5
9.	131	+ 1.3
10.	230	+ 1.1
11.	53	− .1
12.	54	− .6
13.	157	− 2.5
14.	230	− 4.6
15.	43	− 5.4
16.	53	− 6.1
17.	230	−10.5
18.	21	−10.9
19.	157	−11.0
20.	105	−13.0
21.	230	−13.1
22.	230	−14.2
23.	69	−18.7
24.	126	−27.1

For easy comparison of the two groups we have prepared Figure 1 showing all the records, actual and hypothetical. The chart indicates that where forecasting agencies made gains, even the greatest of these lay within limits equalled by the best of our 24 imaginary records representing random action at random intervals. But the extremer losses of the forecasters tended to exceed the losses registered by the least successful of our 24 records of purely chance operations.

In any attempt at interpreting the significance of the performances of the various stock market forecasters we are embarrased by our inability to determine how often economic developments occur of sufficient importance to justify the revision of forecasts. This is tantamount to admitting that we do not know the true number of independent cases, or items, in the time series representing the various forecasting records. In these circumstances, probable errors for correlation coefficients, or for normal distributions, cannot constitute very exact measures of probability. We do know, however, since we are dealing with weekly publications, that the maximum possible number of forecasting opportunities is 52 a year. We also know that forecasts, on the average, undergo some degree of revision about 7 times a year. The correlation

coefficients and probable errors, which constitute one of our tests of probability, have been worked out on both of these bases.

The record of Forecaster Number 1 was available to us for a period of only two years, during which he did not once change his advice. We therefore omitted his record from consideration in our statistical interpretation on the ground that inferences based on it would be relatively inconclusive. For the correlation test therefore, Forecasters Number 2, 3, 22 and 24 were chosen as representing the best and the worst whose records covered the entire period under analysis. The weekly forecasts of each of these four were correlated with the first differences

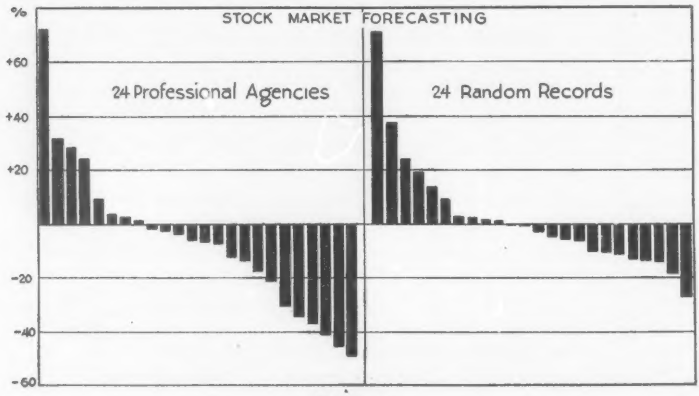

FIGURE 1

of the logarithms of the stock market averages over $4\frac{1}{2}$ years. Forecaster Number 2 had a correlation coefficient of .151; Forecaster Number 3, of .197; and Forecasters Number 22 and 24, of $-.124$ and $-.132$ respectively. The probable error of the best correlation coefficient, with $n=230$, was .043. The difference in r between Forecasters Numbers 2 and 3 is about equal to this probable error, and $r=.197$ is greater than 4 times the probable error of .043. We have interpreted these data by the use of R. A. Fisher's technique, where $z=\tan h^{-1}r$. The best correlation $r=.197$, $n=230$, was first compared with a theoretical $r=.000$, $n=230$; and then compared with $r=-.132$, $n=230$, that is the lowest correlation coefficient.

	r	z	$n-3$	$1/n-3$
1st sample	.197	.200	227	.00441
2nd sample	.000	.000	227	.00441
	Difference	.200	Sum	.00882

$$2\sqrt{.00882}=.188$$

.188 is to be compared with .200. Since these figures are approximately equal, the presumption of skill is slight although the presumption does exist because of the fact that .188 is less than .200, the difference between the two values for z. Using R. A. Fisher's technique in the second case, that is, comparing the best correlation coefficient $r = .197$ with the lowest correlation coefficient $r = -.132$, seems to indicate that there is a real difference between the two samples. If another sample were taken from each of these two forecasters, we should expect to find a similar difference between them in favor of the first forecaster within the limits of the probable error.

We then computed correlation coefficients and probable errors for the data arrived at by taking as our items the periods during which each forecast was in force. We thus had 30 items for Forecaster Number 2, which equalled the number of changes he made in his forecasts, instead of the 230 items which represented the number of weeks for which his record was tabulated. On this basis Forecaster Number 2 had a correlation coefficient of .479; Forecaster Number 3 had a correlation coefficient of .245; and Forecasters Number 22 and 24 had correlation coefficients of $-.513$ and $-.206$ respectively. The probable error of the best correlation coefficient, with $n = 30$, was found to be .095. Thus $r = .479$ was about five times the probable error of .095. The best random forecast had $r = .356 \pm .102$, when his changes of position were taken as the items of the series. When the number of weeks (230), over which his random record extends, was used, and this record correlated with the first differences of the logarithms of the stock market averages, $r = .125 \pm .044$.

Deductions as to the significance of the relationships of the various correlation coefficients to the probable errors are rendered inconclusive, not only by our inability to identify the true number of independent cases in each series, but also by the fact that we have not computed a sufficient number of the correlation coefficients to enable us to determine the character of their distribution.

Having thus experimented with various correlation tests we then resorted to measuring the spread of the performances of the individual forecasters by means of frequency distributions of the percentage weekly gains and losses of each of six forecasters divided by the average result of all possible forecasts. The six chosen were two of the best actual records, two of the worst actual records, and the best and almost the worst random records. The series thus arrived at were each distributed into several classes ranging from 92.50 per cent to 108.50 per cent; these frequencies were plotted and appeared to be reasonably normal. (See Figure 2.) Averages of each of the percentage frequency distributions were computed and compared with a theoretical average

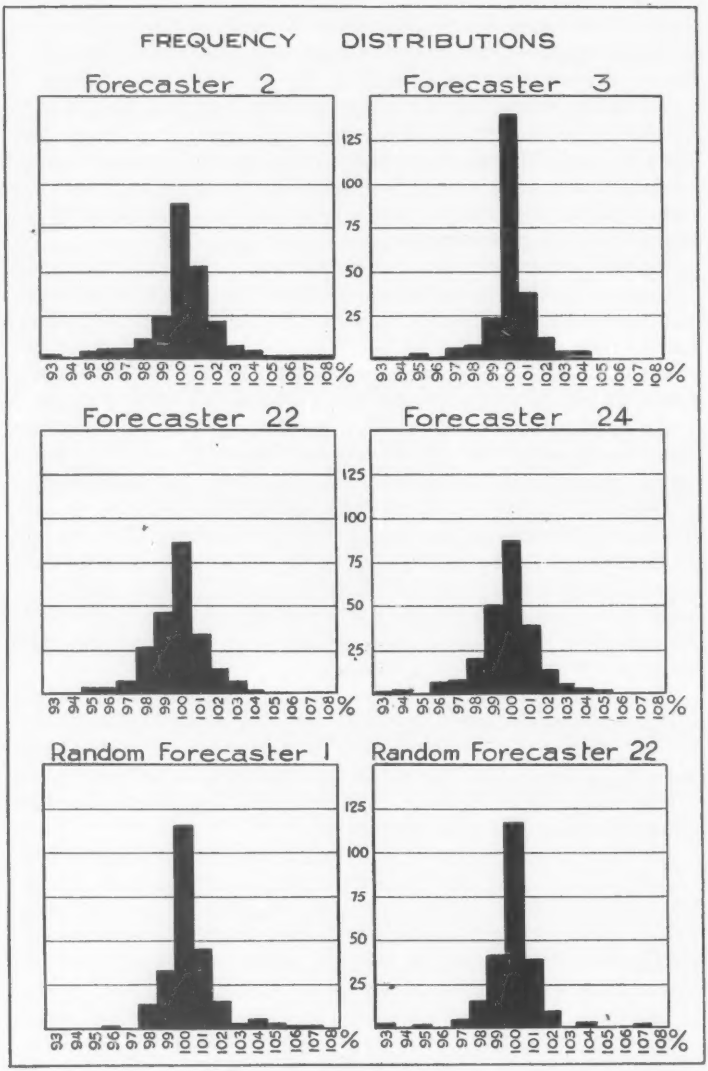
FREQUENCY DISTRIBUTIONS
Forecaster 2
Forecaster 3
Forecaster 22
Forecaster 24
Random Forecaster 1
Random Forecaster 22
125
100
75
50
25
93 94 95 96 97 98 99 100 101 102 103 104 105 106 107 108 %

FIGURE 2

of 100, which represented the average of all random frequency distributions. The probable error of the latter was found to be .086. Forecasters Number 2 and 3 showed averages of 100.098 and 100.103 respectively; each deviating from the theoretical average by amounts which were slightly greater than the probable error .086, but considerably less than twice this probable error. Forecasters Number 22 and 24 had averages of 99.674 and 99.711 respectively; less than the theoretical average by .326 and .289. Each of these differences was more than three, and less than four, times greater than the probable error .086. When a similar frequency distribution is made for the best purely random forecaster, it is found that the average was equal to 100.213, which is greater than that of Forecasters 2 and 3. The deviation from the theoretical average lies within three times the probable error of this theoretical average.

SUMMARY

1. Sixteen financial services, in making some 7500 recommendations of individual common stocks for investment during the period from January 1, 1928, to July 1, 1932, compiled an average record that was worse than that of the average common stock by 1.43 per cent annually. Statistical tests of the best individual records failed to demonstrate that they exhibited skill, and indicated that they more probably were results of chance.

2. Twenty fire insurance companies in making a similar selection of securities during the years 1928 to 1931, inclusive, achieved an average record 1.20 per cent annually worse than that of the general run of stocks. The best of these records, since it is not very much more impressive than the record of the most successful of the sixteen financial services, fails to exhibit definitely the existence of any skill in investment.

3. William Peter Hamilton, editor of the *Wall Street Journal*, publishing forecasts of the stock market based on the Dow Theory over a period of 26 years, from 1904 to 1929, inclusive, achieved a result better than what would ordinarily be regarded as a normal investment return, but poorer than the result of a continuous outright investment in representative common stocks for this period. On 90 occasions he announced changes in the outlook for the market. Forty-five of these predictions were successful and 45 unsuccessful.

4. Twenty-four financial publications engaged in forecasting the stock market during the 4½ years from January 1, 1928, to June 1, 1932, failed as a group by 4 per cent per annum to achieve a result as good as the average of all purely random performances. A review of the various statistical tests, applied to the records for this period, of these 24 fore-

casters, indicates that the most successful records are little, if any, better than what might be expected to result from pure chance. There is some evidence, on the other hand, to indicate that the least successful records are worse than what could reasonably be attributed to chance.

Cowles Commission for Research in Economics

L'UTILITE D'UNE THÉORIE GÉNÉRALE DES ENSEMBLES RENOUVELÉS

PAR FRANÇOIS DIVISIA

Communication lue par titre à la réunion de la Société d'Économétrie, New Orleans, janvier 1932

DANS son ouvrage bien connu, *De la nature du capital et du revenu*, le professeur Irving Fisher a apporté sur une question difficile entre toutes, des clartés précieuses et définitives; il n'est pas interdit de penser que la qualité des résultats de ses travaux en cette matière se ressent beaucoup de ce qu'il a introduit dans la question, des conceptions qui tiennent, de la pensée mathématique, une précision que rien ne saurait remplacer; en particulier, il a mis en évidence que capital et revenu n'ont pas la même dimension, le premier étant un *stock*, c'est-à-dire une quantité de richesses existantes, alors que le second est un *flux*, c'est-à-dire une quantité de richesses, ou mieux de services, considérée pendant un temps donné. Cette notion de la différence de dimension entre deux éléments que l'on pouvait croire comparables, est extrêmement générale et importante; et c'est faute de l'avoir aperçue que des économistes se sont parfois jetés dans des discussions sans issue; on saisit là sur le vif un des aspects de l'avantage que peuvent procurer les mathématiques dans l'étude économique comme ailleurs, celui de préciser les notions, en dehors même de tout calcul proprement dit.

La distinction entre flux et stock a déjà été faite bien souvent; ce sur quoi je voudrais insister ici, c'est que, fréquemment, le même élément peut se présenter sous l'aspect flux et sous l'aspect stock; il y a évidemment nécessité à distinguer soigneusement ces deux aspects; mais il convient aussi de ne pas oublier qu'il peut y avoir quelques relations générales entre eux.

Les phénomènes qui peuvent ainsi se présenter indifféremment sous l'aspect flux, ou sous l'aspect stock, constituent ce que dans un livre intitulé: *L'épargne et la richesse collective*, j'ai appelé *les ensembles renouvelés*, dont les populations humaines sont un cas particulier: dans toute population, il y a *deux flux*, la natalité et la mortalité, et *un stock*, l'effectif; il en est de même dans tout ensemble dont les éléments se renouvellent, par exemple, l'ensemble des individus malades d'une certaine maladie, celui des fonctionnaires d'un certain grade dans une administration, celui des entrepreneurs d'une certaine industrie ou celui des ouvriers employés dans une certaine usine, celui des machines en service, celui des objets approvisionnés dans un magasin, celui des

immeubles d'une ville, celui des sommes déposées dans une banque, celui des obligations d'une société, celui des sommes qui constituent le fonds de roulement d'une entreprise, celui des objets figurant au carnet de commande, celui des objets en cours de fabrication, etc.

Dans tous ces ensembles, on retrouve toujours certains aspects, notamment: (1) Un flux d'entrée, analogue à la natalité; (2) Un flux de sortie, analogue à la mortalité; (3) Un stock d'existants, analogue à l'effectif.

En outre chaque élément de l'ensemble a une durée d'existence dans l'ensemble, et il est souvent possible et commode de définir *la durée moyenne d'existence de tous les éléments* de l'ensemble, analogue à la durée de vie moyenne définie en démographie.

Si l'un des aspects de l'ensemble est intéressant, les autres aspects le sont aussi la plupart du temps, et cela se comprend car ce qui est en cause, en vérité, c'est généralement *l'ensemble lui-même* avec tous ses attributs. En particulier, dans la théorie économique, il faudrait prendre l'habitude de faire intervenir les ensembles renouvelés, ce qui modifierait la présentation de certaines théories et conduirait d'ailleurs naturellement de la statique à la dynamique, puisqu'un ensemble renouvelé se renouvelle nécessairement dans le temps. Cette conception pose la question de savoir comment un ensemble renouvelé peut être défini au point de vue numérique; par un certain nombre de caractéristiques, mais lesquelles? Nous en avons cité un certain nombre, flux d'entrée, flux de sortie, stock d'existants, durée moyenne d'existence, auxquels on peut encore ajouter l'âge moyen des existants; il se peut qu'il y en ait d'autres; d'autre part tous ces élements ne sont pas indépendants.

Par exemple considérons un ensemble dont toutes les caractéristiques demeureraient constantes dans le temps, entrées ou natalité, sorties ou mortalité, existants ou effectif; on sait que si l'on appelle n la natalité, m la mortalité, e l'effectif, d la durée de vie moyenne, on aura: $n=m$ et $e=n.d.$

Lorsque les caractéristiques sont variables dans le temps, les relations de ce genre paraissent plus difficiles à établir, elles n'en existent pas moins, et il serait utile que l'on s'attaquât à ce problème. La démographie a assurément déjà ouvert la voie, mais il ne semble pas qu'elle ait résolu le problème ici en vue; en effet la démographie n'a, à notre connaissance, étudié jusqu'ici par le calcul, que les populations dans lesquelles les éléments s'engendrent les uns les autres, ce qui n'est pas le cas pour l'ensemble renouvelé le plus général; dans cet ensemble en effet, les entrées actuelles peuvent être totalement indépendantes des entrées antérieures. *Une théorie mathématique générale des ensembles renouvelés serait donc utile.*

La connaissance des liaisons mathématiques ci-dessus visées a d'ailleurs un intérêt pratique immédiat; elle habituerait à tenir compte de tous les éléments en cause, par exemple, quand on recense des immeubles, de l'âge moyen de ces immeubles, qui est un élément important de leur valeur; elle permettrait de mieux résoudre certaines difficultés pratiques, par exemple, celles qui se présentent souvent dans l'avancement des fonctionnaires: ce sur quoi l'administration peut agir, c'est sur le flux d'entrée, en ouvrant plus ou moins le robinet du recrutement; ce qui l'intéresse, c'est le stock d'existants qui assurent le service, et comme elle n'a pas une idée nette des liens mathématiques qui peuvent exister entre ces deux caractéristiques, il en résulte des fluctuations, qui tantôt font que les services sont mal assurés, tantôt, produisent dans les cadres un encombrement tel que l'avancement de nombre de fonctionnaires en est compromis. On pourrait citer bien d'autres exemples du même genre; la question a donc un intérêt directement pratique en même temps qu'un intérêt théorique, toutefois, ce dernier l'emporte sans doute de beaucoup.

En définitive, je crois qu'il serait utile: (1°) de faire une étude mathématique générale des ensembles renouvelés, mettant en évidence, d'une part les diverses caractéristiques quantitatives de tels ensembles, d'autre part, les relations générales qui existent entre ces caractéristiques; (2°) d'introduire la considération de ces ensembles dans la théorie économique, pour toutes les questions qui mettent en cause de tels ensembles.

École Nationale des Ponts
et Chaussées

LES SYSTEMES AUTOENTRETENUS ET LES OSCILLATIONS DE RELAXATION

Par Ph. Le Corbeiller

Communication présentée à la réunion de la Société d'Économétrie, Lausanne, septembre 1931.

1.—La recherche de modèles mécaniques des phénomènes électriques a été une des caractéristiques de l'école anglaise de physique au cours du siècle dernier. Depuis une quinzaine d'années cette méthode connaît, sous une forme nouvelle, des succès industriels considérables. Les ingénieurs électriciens, téléphonistes en particulier, s'étant avancés fort loin dans l'étude des réseaux parcourus par des courants alternatifs, furent conduits à traiter les problèmes de mécanique et d'acoustique qu'ils rencontraient au moyen des méthodes qui leur étaient familières. Les industries acoustiques (phonographe, cinéma parlant, . . .) en reçurent en quelques années un développement technique et économique extraordinaire; les industries mécaniques, qui ont derrière elles un long passé, opposent à la transformation plus d'inertie, mais ont commencé cependant à évoluer (témoin l'œuvre entière de M. Constantinesco). L'idée tend même à se répandre que les méthodes que le hasard a fait naître parmi les ingénieurs téléphonistes doivent se constituer en un corps de doctrine, la *Théorie des Oscillations*, dont les théorèmes seraient en principe indépendants des applications mécaniques, thermodynamiques, électriques, chimiques, physiologiques, biologiques, ou économiques, qu'ils pourraient avoir, mais seraient illustrés par des exemples pris dans tous ces domaines. Déjà, en Allemagne, le "Heinrich-Hertz Institut für Schwingungsforschung," dirigé par le Dr. K.-W. Wagner, étudie dans cet esprit les techniques mécanique, électrique, et acoustique. En Économie Politique, l'idée de représenter des phénomènes par des modèles mécaniques n'est pas nouvelle; on voit que l'on pourrait tout aussi bien, peut-être mieux, les représenter par des modèles électriques, voire chimiques ou biologiques. Ce ne seraient là que des traductions, en langages divers, des équations régissant un même groupe de phénomènes, et ce n'est pas dans cette Société qu'il y a lieu de démontrer les avantages de la méthode mathématique.

Le problème des crises, et plus généralement des oscillations des prix, est assurément l'un des plus difficiles de l'Économie Politique; il ne sera sans doute pas de trop, pour approcher de sa solution, de la mise en commun de toutes les ressources de la théorie des oscillations et de la théorie économique. C'est pourquoi j'ai pensé pouvoir vous présenter un compte-rendu succinct d'une avance récente, que je crois

importante, de la théorie des oscillations: celle apportée au problème des systèmes autoentretenus par la découverte des oscillations de relaxation, due à un savant hollandais, le Dr. Balth. van der Pol.

2.—Les oscillations qui ont surtout été étudiées jusqu'à présent sont les *oscillations sinusoïdales* (ou harmoniques, ou pendulaires), dont le type est donné par les petites oscillations du pendule théorique, c'est-à-dire non amorti. Un pendule réel, écarté de sa position d'équilibre et abandonné à lui-même, exécute des oscillations qui vont en s'amortissant en raison des frottements et de la résistance de l'air. Supposons que les causes d'amortissement soient tellement réduites, que pendant une minute, par exemple, l'amplitude des oscillations n'ait pas décru d'une manière appréciable; alors, dans cet intervalle, les variations de l'amplitude en fonction du temps seront représentées par la courbe bien connue, appelée sinusoïde, et données par la formule $a = A \sin \omega t$.

M. van der Pol a donné le nom *d'oscillations de relaxation* à des oscillations d'allure toute différente dont le prototype est donné par la charge lente d'un condensateur, suivie de sa décharge rapide, le cycle se répétant indéfiniment. C'est l'expérience, classique dans les cours de physique élémentaire, des étincelles périodiques qui se produisent lorsqu'on tourne avec une vitesse constante la manivelle d'une machine de Whimshurst. Un exemple mécanique tout semblable est fourni par les machines à battre les pilotis; sous l'action de la vapeur poussant un piston, une lourde masse de fonte est hissée le long d'un poteau; lorsqu'elle atteint une hauteur déterminée, elle ouvre un robinet qui laisse échapper la vapeur, et retombe sur la tête du pilotis.

3.—Dans les deux exemples précédents, on remarque qu'une source d'énergie (bras tournant la manivelle, vapeur poussant le piston) joue un rôle essentiel; elle fournit, pendant la "charge," une certaine quantité d'énergie qui est entièrement dissipée au cours de la "décharge." Au contraire dans le pendule sans amortissement il n'y a pas, par définition, d'énergie dissipée.

Mais précisément ce dernier cas, purement théorique, ne peut être réalisé (impossibilité du mouvement perpétuel). Et un pendule réel n'est pas un système périodique: ses oscillations s'amortissent, et au bout d'un temps plus ou moins long il parvient au repos et y demeure indéfiniment.

Pour que les oscillations d'un pendule soient périodiques comme dans une horloge, il faut qu'elles soient *entretenues*. Cela exige qu'on dispose d'une source d'énergie. Cette source sera par exemple un poids et nous remarquons que ce poids met longtemps, une semaine par exemple, à descendre, ce qui nous indique d'une part qu'il fournit bien l'énergie nécessaire à compenser l'énergie dissipée dans les frottements, d'autre part que cette énergie est, par période, extrêmement faible vis-à-vis de

l'énergie qu'il faudrait pour soulever la lentille du pendule d'une hauteur égale à celle dont elle oscille effectivement.

Si donc nous considérons d'une part une horloge, d'autre part une machine à battre les pilotis, nous voyons que dans l'un et l'autre cas il y a une source d'énergie qui entretient un mouvement périodique en fournissant l'énergie dissipée au cours de chaque période. Dans le cas de l'horloge la lentille effectue des oscillations sinusoïdales (ou presque), et l'énergie fournie pendant une période est très petite vis-à-vis de la quantité d'énergie qui, dans chaque période, oscille sans se dissiper entre les formes dites "cinétique" et "potentielle." Dans le cas de la machine à battre les pilotis, la masse de fonte effectue des oscillations de relaxation, et l'énergie fournie pendant une période est égale à l'énergie cinétique dissipée dans le choc.

M. van der Pol a exprimé cela sous une forme mathématique remarquable, en étudiant graphiquement les solutions périodiques de l'équation différentielle non-linéaire

$$y'' - \epsilon(1 - y^2)y' + y = 0.$$

Le paramètre ϵ caractérise le système; si ϵ est très petit vis-à-vis de l'unité, les oscillations sont sinusoïdales; si ϵ est très grand, ce sont des oscillations de relaxation.

4.—En dehors des oscillations précédentes, la pratique amène aussi à considérer des oscillations d'un autre type, dites *oscillations forcées*.

Un système A effectue des oscillations forcées lorsqu'il oscille sous l'influence d'un autre système oscillant B, lequel n'est, par contre, aucunement influencé par le système A. Posons par exemple un moteur électrique sur une table; la table se mettra à vibrer à la fréquence n, si le moteur tourne à raison de n tours par seconde. Le système A est la table, qui effectue des oscillation forcées; le système B est constitué par l'usine électrique, plus tout le réseau qu'elle dessert avec tous ses organes d'utilisation, parmi lesquels le moteur qui est sur la table. Des cas analogues se rencontrent fréquemment dans la pratique et leur théorie, dans le cas sinusoïdal, est bien connue et très simple; leur particularité la plus remarquable est le phénomène de la *résonance*, qui se produit par exemple si la table, en raison de sa masse et de l'élasticité de ses pieds, a une *fréquence propre* égale justement à n; elle se mettra alors à vibrer avec une très grande amplitude et il pourra même arriver que les pieds se brisent, s'ils sont encastrés.

5.—Pour que le système B impose ses vibrations au système A, (d'où le nom d'oscillations *forcées*) il faut essentiellement que le système A ne réagisse sur B que d'une manière absolument négligeable, ce qui dans la pratique résulte le plus souvent de ce que B est à la fois très gros et très éloigné.

Tel est le cas dans l'exemple précédent. Mais si par contre nous posons une horloge sur une table peu rigide, celle-ci exécutera des oscillations qui troubleront celles du pendule et vice-versa. Il est fort possible, dans un cas semblable, soit que l'horloge s'arrête, soit que le mouvement final de tout le système n'admette pas une période unique. On est donc dans un cas bien plus compliqué que celui des oscillations forcées.

On voit que l'étude d'un système complètement clos, contenant une source d'énergie et dont les différentes parties réagissent les unes sur les autres, sera un problème d'une extrême difficulté; et tel est vraisemblablement le cas du système économique.

Le cas le plus simple est celui où les diverses parties du système oscillent à une fréquence unique. On dit alors qu'on à affaire à un *système oscillant autoentretenu*. Une horloge, une machine à battre les pilotis, en sont justement deux exemples; et la théorie de pareils systèmes repose essentiellement sur l'équation de M. van der Pol écrite plus haut.

6.—L'intérêt de ces remarques pour l'Économie Politique paraît être le suivant. Si l'observation statistique conduit à penser qu'une grandeur déterminée varie périodiquement avec le temps, et qu'on recherche la *cause* de ces oscillations, on peut supposer que cette grandeur effectue, soit (a) des oscillations forcées, soit (b) des oscillations autoentretenues, lesquelles pourront être soit (bα) sinusoïdales, soit (bβ) de relaxation. Encore sont-ce là les cas les plus simples que la réalité puisse présenter.

Une oscillation forcée provient de l'influence d'une cause périodique imperturbable; les variations saisonnières en sont un exemple typique.

Une oscillation autoentretenue suppose (b_1) une source d'énergie, (b_2) un organe de commande à fonctionnement non-linéaire, et (b_3) un organe oscillant ayant inertie, viscosité et raideur. Cette complication, qui peut effrayer, est aussi inévitable en Économie Politique qu'en horlogerie ou en radioélectricité.

Toutefois il y a lieu d'observer que si l'oscillation est sinusoïdale (bα), le système total se comporte à très peu près comme un organe oscillant isolé ayant seulement inertie et raideur; et si l'oscillation est de relaxation (bβ), le système se comporte à très peu près comme si l'organe entretenu était dénué soit d'inertie, soit de raideur. De plus, la forme des courbes d'oscillation permet de présumer à quel type d'oscillation on a affaire: si les courbes sont régulièrement sinueuses on a probablement une oscillation sinusoïdale, si elles présentent par période deux discontinuités (ou deux points anguleux) on a probablement une oscillation de relaxation.

7.—Il m'est impossible d'entrer davantage ici dans les détails de ce sujet; je me borne donc à donner les deux références suivantes, qui contiennent de nombreuses indications bibliographiques:

B. van der Pol: "Oscillations sinusoïdales et de relaxation." *L'Onde Électrique*, juin-juillet 1930 (Chiron, éd. à Paris).

Ph. Le Corbeiller: "Les systèmes autoentretenus et les oscillations de relaxation." *Conférences du Conservatoire national des Arts et Métiers* (Hermann, éd. à Paris, 1931).

Conservatoire National des Arts et Métiers, Paris

PROGRAM OF THE MEETING OF THE ECONOMETRIC SOCIETY IN CHICAGO, JUNE 28–30, 1933

WEDNESDAY, JUNE 28, 10 A.M., in the Social Science Assembly Room, Social Science Building, University of Chicago; topic, "Elasticities of Demand and Supply"; chairman, Col. Edward N. Wentworth, Armour's Livestock Bureau, Chicago; speakers, Sir Daniel Hall, Director of the John Innes Horticultural Institution, Merton, England, "Elasticities of Demand for Agricultural Products"; Professor Harry Pelle Hartkemeir, School of Business and Public Administration, University of Missouri, "Elasticity of Supply for Agricultural Products, with Special Application to Corn and Potatoes"; Dr. Roswell Whitman, of the Illinois Emergency Relief Commission, "Elasticity of Demand for Producers' Goods as Illustrated by the Demand for Iron and Steel"; Mr. L. H. Bean, U. S. Department of Agriculture, "Elasticity of Demand for Agricultural Products with Special Application to Perishables."

Thursday, June 29, 10 A.M., in the Social Science Assembly Room, Social Science Building, University of Chicago; topic, "Business Cycle Theory"; chairman, Professor Wesley C. Mitchell of Columbia University; speakers, Dr. Norman J. Silberling of San Francisco, "The Monetary Factor in Business Cycles"; Professor Harold T. Davis of Indiana University and the Cowles Commission for Research in Economics, "Contributions to the Theory of the Harmonic Analysis of Economic Series"; Professor James G. Smith of Princeton University, "The Impact of Business Cycle Theory upon Political Thought and Action"; Professor Irving Fisher of Yale University, President of the Econometric Society, "The Debt Theory of Depressions"; Professor Ragnar Frisch of the University of Norway, Editor of ECONOMETRICA, "Changing Harmonics Studied from the Point of View of Linear Operators and Erratic Shocks" (to be read by title), Dr. Carl Snyder, of the Federal Reserve Bank of New York, "Real Wages and the Per Capita Growth of Industrial Product: A Social Problem."

Friday, June 30, 2 P.M., Room 405, Palmer House, Chicago; meeting sponsored by the Econometric Society, the American Society of Mechanical Engineers, The American Society for Testing Materials, and the American Institute of Electrical Engineers; topic "Some Fundamental Problems of Mutual Interest to Scientific Economists and Engineers"; chairman, Professor Irving Fisher of Yale University, President of the Econometric Society; speakers, Dr. Charles F. Roos, Secretary of the Econometric Society, "Contributions of the Mathematician

to Economics," discussion led by Professor Henry Schultz of the University of Chicago, and one other; Dr. Thornton C. Fry of the Bell Telephone Laboratories, "The Mathematical Theory of Rational Inference," discussion led by Professor Harold Hotelling of Columbia University, Mr. L. K. Sillcox, Vice-President of the New York Air Brake Company, and Dr. Anson Hayes, Director of Research of the American Rolling Mill Company; Dean Dexter S. Kimball of Cornell University, "The Engineering Economist of the Future," discussion led by President Harvey N. Davis of Stevens Institute of Technology, and Mr. C. F. Hirshfeld, Director of Research of the Detroit Edison Company.

Friday, June 30, 8 P.M., Red Lacquer Room, Palmer House, Chicago; chairman, Mr. Ralph E. Flanders, Chairman of the Committee on Economic Balance of the American Engineering Council; honorary chairmen, presidents of the sponsoring societies; speakers, Dr. F. B. Jewett, Vice-President of the American Telephone and Telegraph Company, "Some Fundamental Problems of the Engineer"; Dr. A. P. M. Fleming, Director of Research and Education of the Metropolitan-Vickers Electrical Company, Manchester, England, "The Internationalization of Scientific Knowledge as a Factor in World Economic Recovery."

* * * *

Professor Henry Schultz of the University of Chicago will be in charge of local arrangements for these meetings of the Econometric Society and may be solicited for further information. Those in attendance may secure final programs at the headquarters of the Society, Room 101, Social Science Building, University of Chicago. The American Association for the Advancement of Science, with headquarters in the Hotel Stevens at Chicago, will also include in its program details of the Econometric Society meetings.

AVIS DE LA RÉUNION DE LA SOCIÉTÉ D'ÉCONOMÉTRIE, A LEYDE, HOLLANDE, OCTOBRE, 1933

La 3e réunion européenne de la Société internationale d'Économétrie se tiendra à Leyde (Pays-Bas) vers le début d'Octobre.

Le programme des travaux, qui n'est pas encore arrêté, sera conçu dans le même esprit que ceux des précédentes réunions de Lausanne 1931 et Paris 1932 (voir ECONOMETRICA, n° de Janvier 1933, et *Revue d'Économie Politique*, n° de Janvier 1933).

Les membres qui envisagent de participer à la réunion sont priés de le faire connaître dès que possible à Monsieur J. Tinbergen, Tesselsche straat 39, SCHEVENINGEN (Hollande), qui a bien voulu se charger de recueillir les adhésions et leur fera parvenir les circulaires ultérieures.

Ils sont priés, en outre, d'indiquer leurs convenances quant à la fixation des jours de session, et de faire connaître s'ils désirent participer à un arrangement collectif pour le logement et les repas, en vue de réduire les frais de séjour.

Les adhésions devront être accompagnées du versement de deux florins pour la couverture des frais d'organisation.

Les membres désireux de proposer une communication devront en adresser le texte ou tout au moins une analyse assez détaillée à Monsieur Zeuthen, Steen Blichersvej 1, COPENHAGUE (Danemark), le plus tôt possible.

Le Vice-Président,
F. DIVISIA.

30 Avril 1933.

PAPERS TO APPEAR IN ECONOMETRICA

THE SURVEYS

JAKOB MARSCHAK: Significant Developments in Statistical Information (to appear October, 1933)

J. TINBERGEN: Significant Developments in General Economic Theory (to appear January, 1934)

G. DARMOIS: Significant Developments in Statistical Technique (to appear July, 1934)

J. R. HICKS, Significant Developments in General Economic Theory (to appear January, 1935).

OTHER CONTRIBUTIONS TO APPEAR IN EARLY ISSUES

LUIGI AMOROSO: Vilfredo Pareto

PROF. BONINSEGNI: Léon Walras

A. L. BOWLEY: Francis Ysidro Edgeworth

A. L. BOWLEY: The Action of Economic Forces in Producing Frequency Distributions of Income, Prices, and Other Phenomena: A Suggestion for study.

IRVING FISHER: The Income Concept

RAGNAR FRISCH: Changing Harmonics Studied from the Point of View of Linear Operators and Erratic Shocks

RAGNAR FRISCH and FREDERICK V. WAUGH: Partial Time Regressions as Compared with Individual Trends

L. FURLAN: Le volume maximum du crédit dans le cas d'un système économique isolé

RAYMOND GARVER: The Edgeworth Taxation Phenomenon

PH. LE CORBEILLER: Oscillations autoentretenus dans le cas discontinu

GEORGES LUTFALLA: Fisher et Laurent, inventeurs des fonctions d'indice

JAKOB MARSCHAK: Econometric Parameters in a Stationary Society with Monetary Circulation.

JOSEPH MAYER: Pseudo-scientific Method in Economics

E. H. PHELPS BROWN: Demand Functions and Utility Functions. A Critical Examination of Their Meaning.

ERICH SCHNEIDER: Statistical Construction of Cost Curves

ERICH SCHNEIDER: Heinrich von Thünen

EUGENE SLUTSKY: The Summation of Random Causes as a Source of Cyclic Processes

HANS STAEHLE: The Reaction of Consumers to Changes in Prices and Income. A Quantitative Study in Immigrants' Behaviour.

List of Fellows of the Econometric Society

THE DEBT-DEFLATION THEORY OF GREAT DEPRESSIONS

By Irving Fisher

INTRODUCTORY

In *Booms and Depressions*, I have developed, theoretically and statistically, what may be called a debt-deflation theory of great depressions. In the preface, I stated that the results "seem largely new," I spoke thus cautiously because of my unfamiliarity with the vast literature on the subject. Since the book was published its special conclusions have been widely accepted and, so far as I know, no one has yet found them anticipated by previous writers, though several, including myself, have zealously sought to find such anticipations. Two of the best-read authorities in this field assure me that those conclusions are, in the words of one of them, "both new and important."

Partly to specify what some of these special conclusions are which are believed to be new and partly to fit them into the conclusions of other students in this field, I am offering this paper as embodying, in brief, my present "creed" on the whole subject of so-called "cycle theory." My "creed" consists of 49 "articles" some of which are old and some new. I say "creed" because, for brevity, it is purposely expressed dogmatically and without proof. But it is not a creed in the sense that my faith in it does not rest on evidence and that I am not ready to modify it on presentation of new evidence. On the contrary, it is quite tentative. It may serve as a challenge to others and as raw material to help them work out a better product.

Meanwhile the following is a list of my 49 tentative conclusions.

"CYCLE THEORY" IN GENERAL

1. The economic system contains innumerable variables—quantities of "goods" (physical wealth, property rights, and services), the prices of these goods, and their values (the quantities multiplied by the prices). Changes in any or all of this vast array of variables may be due to many causes. Only in imagination can all of these variables remain constant and be kept in equilibrium by the balanced forces of human desires, as manifested through "supply and demand."

2. Economic theory includes a study both of (a) such imaginary, ideal equilibrium—which may be stable or unstable—and (b) dis-equilibrium. The former is economic statics; the latter, economic dynamics. So-called cycle theory is merely one part of the study of economic dis-equilibrium.

3. The study of dis-equilibrium may proceed in either of two ways.

We may take as our unit for study an actual historical case of great dis-equilibrium, such as, say, the panic of 1873; or we may take as our unit for study any constituent tendency, such as, say, deflation, and discover its general laws, relations to, and combinations with, other tendencies. The former study revolves around events, or *facts;* the latter, around *tendencies.* The former is primarily economic history; the latter is primarily economic science. Both sorts of studies are proper and important. Each helps the other. The panic of 1873 can only be understood in the light of the various tendencies involved—deflation and other; and deflation can only be understood in the light of the various historical manifestations—1873 and other.

4. The old and apparently still persistent notion of "the" business cycle, as a single, simple, self-generating cycle (analogous to that of a pendulum swinging under influence of the single force of gravity) and as actually realized historically in regularly recurring crises, is a myth. Instead of one force there are many forces. Specifically, instead of one cycle, there are many co-existing cycles, constantly aggravating or neutralizing each other, as well as co-existing with many non-cyclical forces. In other words, while a cycle, conceived as a *fact,* or historical event, is non-existent, there are always innumerable cycles, long and short, big and little, conceived as *tendencies* (as well as numerous non-cyclical tendencies), any historical event being the resultant of all the tendencies then at work. Any one cycle, however perfect and like a sine curve it may tend to be, is sure to be interfered with by other tendencies.

5. The innumerable tendencies making mostly for economic dis-equilibrium may roughly be classified under three groups: (a) growth or trend tendencies, which are steady; (b) haphazard disturbances, which are unsteady; (c) cyclical tendencies, which are unsteady but steadily repeated.

6. There are two sorts of cyclical tendencies. One is "forced" or imposed on the economic mechanism from outside. Such is the yearly rhythm; also the daily rhythm. Both the yearly and the daily rhythm are imposed on us by astronomical forces from outside the economic organization; and there may be others such as from sun spots or transits of Venus. Other examples of "forced" cycles are the monthly and weekly rhythms imposed on us by custom and religion.

The second sort of cyclical tendency is the "free" cycle, not forced from outside, but self-generating, operating analogously to a pendulum or wave motion.

7. It is the "free" type of cycle which is apparently uppermost in the minds of most people when they talk of "the" business cycle. The yearly cycle, though it more nearly approaches a perfect cycle than

any other, is seldom thought of as a cycle at all but referred to as "seasonal variation."

8. There may be equilibrium which, though stable, is so delicately poised that, after departure from it beyond certain limits, instability ensues, just as, at first, a stick may bend under strain, ready all the time to bend back, until a certain point is reached, when it breaks. This simile probably applies when a debtor gets "broke,"or when the breaking of many debtors constitutes a "crash," after which there is no coming back to the original equilibrium. To take another simile, such a disaster is somewhat like the "capsizing" of a ship which, under ordinary conditions, is always near stable equilibrium but which, after being tipped beyond a certain angle, has no longer this tendency to return to equilibrium, but, instead, a tendency to depart further from it.

9. We may tentatively assume that, ordinarily and within wide limits, all, or almost all, economic variables tend, in a general way, toward a stable equilibrium. In our classroom expositions of supply and demand curves, we very properly assume that if the price, say, of sugar is above the point at which supply and demand are equal, it tends to fall; and if below, to rise.

10. Under such assumptions, and taking account of "economic friction," which is always present, it follows that, unless some outside force intervenes, any "free" oscillations about equilibrium must tend progressively to grow smaller and smaller, just as a rocking chair set in motion tends to stop. That is, while "forced" cycles, such as seasonal, tend to continue unabated in amplitude, ordinary "free" cycles tend to cease, giving way to equilibrium.

11. But the exact equilibrium thus sought is seldom reached and never long maintained. New disturbances are, humanly speaking, sure to occur, so that, in actual fact, any variable is almost always above or below the ideal equilibrium.

For example, coffee in Brazil may be over-produced, that is, may be more than it would have been if the producers had known in advance that it could not have been sold at a profit. Or there may be a shortage in the cotton crop. Or factory, or commercial inventories may be under or over the equilibrium point.

Theoretically there may be—in fact, at most times there must be—over- or under-production, over- or under-consumption, over- or under-spending, over- or under-saving, over- or under-investment, and over or under everything else. It is as absurd to assume that, for any long period of time, the variables in the economic organization, or any part of them, will "stay put," in perfect equilibrium, as to assume that the Atlantic Ocean can ever be without a wave.

12. The important variables which may, and ordinarily do, stand

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above or below equilibrium are: (a) capital items, such as homes, factories, ships, productive capacity generally, inventories, gold, money, credits, and debts; (b) income items, such as real income, volume of trade, shares traded; (c) price items, such as prices of securities, commodities, interest.

13. There may even be a *general* over-production and in either of two senses: (a) there may be, in general, at a particular point of time, over-large inventories or stocks on hand, or (b) there may be, in general, during a particular period of time, an over-rapid flow of production. The classical notion that over-production can only be relative as between different products is erroneous. Aside from the abundance or scarcity of particular products, relative to each other, production as a whole is relative to human desires and aversions, and can as a whole overshoot or undershoot the equilibrium mark.

In fact, except for brief moments, there must always be some degree of general over-production or general under-production and in both senses—stock and flow.

14. But, in practice, general over-production, as popularly imagined, has never, so far as I can discover, been a chief cause of great dis-equilibrium. The reason, or a reason, for the common notion of over-production is mistaking too little money for too much goods.

15. While any deviation from equilibrium of any economic variable theoretically may, and doubtless in practice does, set up some sort of oscillations, the important question is: Which of them have been sufficiently great disturbers to afford any substantial explanation of the great booms and depressions of history?

16. I am not sufficiently familiar with the long detailed history of these disturbances, nor with the colossal literature concerning their alleged explanations, to have reached any definitive conclusions as to the relative importance of all the influences at work. I am eager to learn from others.

17. According to my present opinion, which is purely tentative, there is some grain of truth in most of the alleged explanations commonly offered, but this grain is often small. Any of them may suffice to explain *small* disturbances, but all of them put together have probably been inadequate to explain *big* disturbances.

18. In particular, as explanations of the so-called business cycle, or cycles, when these are really serious, I doubt the adequacy of over-production, under-consumption, over-capacity, price-dislocation, maladjustment between agricultural and industrial prices, over-confidence, over-investment, over-saving, over-spending, and the discrepancy between saving and investment.

19. I venture the opinion, subject to correction on submission of

future evidence, that, in the great booms and depressions, each of the above-named factors has played a subordinate rôle as compared with two dominant factors, namely *over-indebtedness* to start with and *deflation* following soon after; also that where any of the other factors do become conspicuous, they are often merely effects or symptoms of these two. In short, the big bad actors are debt disturbances and price-level disturbances.

While quite ready to change my opinion, I have, at present, a strong conviction that these two economic maladies, the debt disease and the price-level disease (or dollar disease), are, in the great booms and depressions, more important causes than all others put together.

20. Some of the other and usually minor factors often derive some importance when combined with one or both of the two dominant factors.

Thus over-investment and over-speculation are often important; but they would have far less serious results were they not conducted with borrowed money. That is, over-indebtedness may lend importance to over-investment or to over-speculation.

The same is true as to over-confidence. I fancy that over-confidence seldom does any great harm except when, as, and if, it beguiles its victims into debt.

Another example is the mal-adjustment between agricultural and industrial prices, which can be shown to be a result of a change in the general price level.

21. Disturbances in these two factors—debt and the purchasing power of the monetary unit—will set up serious disturbances in all, or nearly all, other economic variables. On the other hand, if debt and deflation are absent, other disturbances are powerless to bring on crises comparable in severity to those of 1837, 1873, or 1929-33.

THE ROLES OF DEBT AND DEFLATION

22. No exhaustive list can be given of the secondary variables affected by the two primary ones, debt and deflation; but they include especially seven, making in all at least nine variables, as follows: debts, circulating media, their velocity of circulation, price levels, net worths, profits, trade, business confidence, interest rates.

23. *The chief interrelations between the nine chief factors may be derived deductively*, assuming, to start with, that general economic equilibrium is disturbed by only the one factor of over-indebtedness, and, in particular, assuming that there is no other influence, whether accidental or designed, tending to affect the price level.

24. Assuming, accordingly, that, at some point of time, a state of over-indebtedness exists, this will tend to lead to liquidation, through

the alarm either of debtors or creditors or both. Then we may deduce the following chain of consequences in nine links: (1) *Debt liquidation* leads to *distress selling* and to (2) *Contraction of deposit currency*, as bank loans are paid off, and to a slowing down of velocity of circulation. This contraction of deposits and of their velocity, precipitated by distress selling, causes (3) *A fall in the level of prices*, in other words, a swelling of the dollar. Assuming, as above stated, that this fall of prices is not interfered with by reflation or otherwise, there must be (4) *A still greater fall in the net worths of business*, precipitating bankruptcies and (5) *A like fall in profits*, which in a "capitalistic," that is, a private-profit society, leads the concerns which are running at a loss to make (6) *A reduction in output, in trade and in employment* of labor. These losses, bankruptcies, and unemployment, lead to (7) *Pessimism and loss of confidence*, which in turn lead to (8) *Hoarding and slowing down still more the velocity of circulation.*

The above eight changes cause (9) *Complicated disturbances in the rates of interest*, in particular, a fall in the nominal, or money, rates and a rise in the real, or commodity, rates of interest.

Evidently debt and deflation go far toward explaining a great mass of phenomena in a very simple logical way.

25. The above chain of causes, consisting of nine links, includes only a few of the interrelations between the nine factors. There are other demonstrable interrelations, both rational and empirical, and doubtless still others which cannot, yet, at least, be formulated at all.[1] There must also be many indirect relations involving variables not included among the nine groups.

26. One of the most important of such interrelations (and probably too little stressed in my *Booms and Depressions*) is the direct effect of lessened money, deposits, and their velocity, in curtailing trade, as evidenced by the fact that trade has been revived locally by emergency money without any raising of the price level.

27. In actual chronology, the order of the nine events is somewhat different from the above "logical" order, and there are reactions and repeated effects. As stated in Appendix I of *Booms and Depressions:*

> The following table of our nine factors, occurring and recurring (together with distress selling), gives a fairly typical, though still inadequate, picture of the

[1] Many of these interrelations have been shown statistically, and by many writers. Some, which I have so shown and which fit in with the debt-deflation theory, are: that price-change, after a distributed lag, causes, or is followed by, corresponding fluctuations in the volume of trade , employment, bankruptcies, and rate of interest. The results as to price-change and unemployment are contained in Charts II and III, pp. 352–3. See references at the end of this article; also footnote 2, page 345, regarding the charts.

cross-currents of a depression in the approximate order in which it is believed they usually occur. (The first occurrence of each factor and its sub-divisions is indicated by italics. The figures in parenthesis show the sequence in the original exposition.)

I. (7) Mild *Gloom* and Shock to *Confidence*
(8) Slightly *Reduced Velocity* of Circulation
(1) Debt *Liquidation*

II. (9) *Money Interest* on Safe Loans Falls
(9) But Money Interest on Unsafe Loans Rises

III. (2) *Distress Selling*
(7) More Gloom
(3) *Fall in Security Prices*
(1) More Liquidation
(3) *Fall in Commodity Prices*

IV. (9) *Real Interest Rises;* REAL DEBTS INCREASE
(7) More Pessimism and Distrust
(1) More Liquidation
(2) More Distress Selling
(8) More Reduction in Velocity

V. (2) More Distress Selling
(2) *Contraction of Deposit Currency*
(3) Further Dollar Enlargement

VI. (4) *Reduction in Net Worth*
(4) Increase in *Bankruptcies*
(7) More Pessimism and Distrust
(8) More Slowing in Velocity
(1) More Liquidation

VII. (5) *Decrease in Profits*
(5) *Increase in Losses*
(7) Increase in Pessimism
(8) Slower Velocity
(1) More Liquidation
(6) *Reduction in Volume of Stock Trading*

VIII. (6) *Decrease in Construction*
(6) *Reduction in Output*
(6) *Reduction in Trade*
(6) *Unemployment*
(7) More Pessimism

IX. (8) *Hoarding*

X. (8) *Runs on Banks*
(8) *Banks Curtailing Loans* for Self-Protection
(8) *Banks Selling Investments*
(8) *Bank Failures*
(7) Distrust Grows
(8) More Hoarding
(1) More Liquidation
(2) More Distress Selling
(3) Further Dollar Enlargement

As has been stated, this order (or any order, for that matter) can be only approximate and subject to variations at different times and places. It represents my present guess as to how, if not too much interfered with, the nine factors selected for explicit study in this book are likely in most cases to fall in line.

But, as has also been stated, the idea of a single-line succession is itself inadequate, for while Factor (1) acts on (2), for instance, it also acts *directly* on (7), so that we really need a picture of subdividing streams or, better, an interacting network in which each factor may be pictured as influencing and being influenced by many or all of the others.

Paragraph 24 above gives a logical, and paragraph 27 a chronological, order of the chief variables put out of joint in a depression when once started by over-indebtedness.

28. But it should be noted that, except for the first and last in the "logical" list, namely debt and interest on debts, *all the fluctuations listed come about through a fall of prices.*

29. When over-indebtedness stands alone, that is, does *not* lead to a fall of prices, in other words, when its tendency to do so is counteracted by inflationary forces (whether by accident or design), the resulting "cycle" will be far milder and far more regular.

30. Likewise, when a deflation occurs from other than debt causes and without any great volume of debt, the resulting evils are much less. It is the combination of both—the debt disease coming first, then precipitating the dollar disease—which works the greatest havoc.

31. The two diseases act and react on each other. Pathologists are now discovering that a pair of diseases are sometimes worse than either or than the mere sum of both, so to speak. And we all know that a minor disease may lead to a major one. Just as a bad cold leads to pneumonia, so over-indebtedness leads to deflation.

32. And, vice versa, deflation caused by the debt reacts on the debt. Each dollar of debt still unpaid becomes a bigger dollar, and if the over-indebtedness with which we started was great enough, the liquidation of debts cannot keep up with the fall of prices which it causes. In that case, the liquidation defeats itself. While it diminishes the number of dollars owed, it may not do so as fast as it increases the value of each dollar owed. Then, *the very effort of individuals to lessen their burden of debts increases it, because of the mass effect of the stampede to liquidate in swelling each dollar owed.* Then we have the great paradox which, I submit, is the chief secret of most, if not all, great depressions: *The more the debtors pay, the more they owe.* The more the economic boat tips, the more it tends to tip. It is not tending to right itself, but is capsizing.

33. But if the over-indebtedness is not sufficiently great to make liquidation thus defeat itself, the situation is different and simpler. It is then more analogous to stable equilibrium; the more the boat

rocks the more it will tend to right itself. In that case, we have a truer example of a cycle.

34. In the "capsizing" type in particular, the worst of it is that real incomes are so rapidly and progressively reduced. Idle men and idle machines spell lessened production and lessened real income, the central factor in all economic science. Incidentally this under-production occurs at the very time that there is the illusion of over-production.

35. In this rapid survey, I have not discussed what constitutes over-indebtedness. Suffice it here to note that (a) over-indebtedness is always relative to other items, including national wealth and income and the gold supply, which last is specially important, as evidenced by the recent researches of Warren and Pearson; and (b) it is not a mere one-dimensional magnitude to be measured simply by the number of dollars owed. It must also take account of the distribution in time of the sums coming due. Debts due at once are more embarrassing than debts due years hence; and those payable at the option of the creditor, than those payable at the convenience of the debtor. Thus debt embarrassment is great for call loans and for early maturities.

For practical purposes, we may roughly measure the total national debt embarrassment by taking the total sum currently due, say within the current year, including rent, taxes, interest, installments, sinking fund requirements, maturities and any other definite or rigid commitments for payment on principal.

ILLUSTRATED BY THE DEPRESSION OF 1929-33[2]

36. The depression out of which we are now (I trust) emerging is an example of a debt-deflation depression of the most serious sort. The

[2] Note the charts, pp. 352–7:

Chart I shows: (1) the price level (P) and (2) its percentage rate of rise or fall (P'). When the last named is lagged with the lag distributed according to a probability curve so that the various P''s overlap and cumulate we get $\overline{P}'$, as in Charts II and III. This $\overline{P}'$ is virtually a lagged average of the P''s.

Charts II and *III* show: $\overline{P}'$ contrasted with employment (E). $\overline{P}'$ may be considered as what employment would be if controlled *entirely* by price-change.

Chart IV shows the Swedish official (retail) weekly index number contrasted with the American weekly wholesale and monthly retail indexes.

Chart V shows the estimated internal debt in the United States contrasted with the estimated total money value of wealth. The unshaded extensions of the bars upward show what the 1933 figures would be if enlarged 75 per cent to translate them into 1929 dollars (according to the index number of wholesale commodity prices).

Chart VI shows estimated "fixed" annual charges (actually collected) contrasted with estimated national income. The unshaded extensions of the bars upward show what the 1932 figures would be if enlarged 56 per cent to translate them into 1929 dollars.

Charts VII and *VIII* show the chief available statistics before and after March 4, 1933, grouped in the order indicated in Article 27 above.

debts of 1929 were the greatest known, both nominally and really, up to that time.

They were great enough not only to "rock the boat" but to start it capsizing. By March, 1933, liquidation had reduced the debts about 20 per cent, but had increased the dollar about 75 per cent, so that the *real* debt, that is the debt as measured in terms of commodities, was increased about 40 per cent $[(100\%-20\%)\times(100\%+75\%)=140\%]$. Note Chart V.

37. Unless some counteracting cause comes along to prevent the fall in the price level, such a depression as that of 1929-33 (namely when the more the debtors pay the more they owe) tends to continue, going deeper, in a vicious spiral, for many years. There is then no tendency of the boat to stop tipping until it has capsized. Ultimately, of course, but only after almost universal bankruptcy, the indebtedness must cease to grow greater and begin to grow less. Then comes recovery and a tendency for a new boom-depression sequence. This is the so-called "natural" way out of a depression, via needless and cruel bankruptcy, unemployment, and starvation.

38. On the other hand, if the foregoing analysis is correct, it is always economically possible to stop or prevent such a depression simply by reflating the price level up to the average level at which outstanding debts were contracted by existing debtors and assumed by existing creditors, and then maintaining that level unchanged.

That the price level is controllable is not only claimed by monetary theorists but has recently been evidenced by two great events: (1) Sweden has now for nearly two years maintained a stable price level, practically always within 2 per cent of the chosen par and usually within 1 per cent. Note Chart IV. (2) The fact that immediate reversal of deflation is easily achieved by the use, or even the prospect of use, of appropriate instrumentalities has just been demonstrated by President Roosevelt. Note Charts VII and VIII.

39. Those who imagine that Roosevelt's avowed reflation is not the cause of our recovery but that we had "reached the bottom anyway" are very much mistaken. At any rate, they have given no evidence, so far as I have seen, that we had reached the bottom. And if they are right, my analysis must be woefully wrong. According to all the evidence, under that analysis, debt and deflation, which had wrought havoc up to March 4, 1933, were then stronger than ever and, if let alone, would have wreaked greater wreckage than ever, after March 4. Had no "artificial respiration" been applied, we would soon have seen general bankruptcies of the mortgage guarantee companies, savings banks, life insurance companies, railways, municipalities, and states. By that time the Federal Government would probably have be-

come unable to pay its bills without resort to the printing press, which would itself have been a very belated and unfortunate case of artificial respiration. If even then our rulers should still have insisted on "leaving recovery to nature" and should still have refused to inflate in any way, should vainly have tried to balance the budget and discharge more government employees, to raise taxes, to float, or try to float, more loans, they would soon have ceased to be our rulers. For we would have insolvency of our national government itself, and probably some form of political revolution without waiting for the next legal election. The mid-west farmers had already begun to defy the law.

40. If all this is true, it would be as silly and immoral to "let nature take her course" as for a physician to neglect a case of pneumonia. It would also be a libel on economic science, which has its therapeutics as truly as medical science.

41. If reflation can now so easily and quickly reverse the deadly down-swing of deflation after nearly four years, when it was gathering increased momentum, it would have been still easier, and at any time, to have stopped it earlier. In fact, under President Hoover, recovery was apparently well started by the Federal Reserve open-market purchases, which revived prices and business from May to September 1932. The efforts were not kept up and recovery was stopped by various circumstances, including the political "campaign of fear."

It would have been still easier to have prevented the depression almost altogether. In fact, in my opinion, this would have been done had Governor Strong of the Federal Reserve Bank of New York lived, or had his policies been embraced by other banks and the Federal Reserve Board and pursued consistently after his death.[3] In that case, there would have been nothing worse than the first crash. We would have had the debt disease, but not the dollar disease—the bad cold but not the pneumonia.

42. If the debt-deflation theory of great depressions is essentially correct, the question of controlling the price level assumes a new importance; and those in the drivers' seats—the Federal Reserve Board and the Secretary of the Treasury, or, let us hope, a special stabilization commission—will in future be held to a new accountability.

43. Price level control, or dollar control, would not be a panacea. Even with an ideally stable dollar, we would still be exposed to the

[3] Eventually, however, in order to have avoided depression, the gold standard would have had to be abandoned or modified (by devaluation); for, with the gold standard as of 1929, the price levels at that time could not have been maintained indefinitely in the face of: (1) the "scramble for gold" due to the continued extension of the gold standard to include nation after nation; (2) the increasing volume of trade; and (3) the prospective insufficiency of the world gold supply.

debt disease, to the technological-unemployment disease, to over-production, price-dislocation, over-confidence, and many other minor diseases. To find the proper therapy for these diseases will keep economists busy long after we have exterminated the dollar disease.

DEBT STARTERS

44. The over-indebtedness hitherto presupposed must have had its starters. It may be started by many causes, of which the most common appears to be *new opportunities to invest at a big prospective profit*, as compared with ordinary profits and interest, such as through new inventions, new industries, development of new resources, opening of new lands or new markets. Easy money is the great cause of over-borrowing. When an investor thinks he can make over 100 per cent per annum by borrowing at 6 per cent, he will be tempted to borrow, and to invest or speculate with borrowed money. This was a prime cause leading to the over-indebtedness of 1929. Inventions and technological improvements created wonderful investment opportunities, and so caused big debts. Other causes were the left-over war debts, domestic and foreign, public and private, the reconstruction loans to foreigners, and the low interest policy adopted to help England get back on the gold standard in 1925.

Each case of over-indebtedness has its own starter or set of starters. The chief starters of the over-indebtedness leading up to the crisis of 1837 were connected with lucrative investment opportunities from developing the West and Southwest in real estate, cotton, canal building (led by the Erie Canal), steamboats, and turnpikes, opening up each side of the Appalachian Mountains to the other. For the over-indebtedness leading up to the crisis of 1873, the chief starters were the exploitation of railways and of western farms following the Homestead Act. The over-indebtedness leading up to the panic of 1893 was chiefly relative to the gold base which had become too small, because of the injection of too much silver. But the panic of 1893 seems to have had less of the debt ingredient than in most cases, though deflation played a leading rôle.

The starter may, of course, be wholly or in part the pendulum-like back-swing or reaction in recovery from a preceding depression as commonly assumed by cycle theorists. This, of itself, would tend to leave the next depression smaller than the last.

45. When the starter consists of new opportunities to make unusually profitable investments, the bubble of debt tends to be blown bigger and faster than when the starter is great misfortune causing merely non-productive debts. The only notable exception is a great war and even then chiefly because it leads *after it is over* to productive debts for reconstruction purposes.

46. This is quite different from the common naïve opinions of how war results in depression. If the present interpretation is correct, the World War need never have led to a great depression. It is very true that much or most of the inflations could not have been helped because of the exigencies of governmental finance, but the subsequent undue deflations could probably have been avoided entirely.

47. The public psychology of going into debt for gain passes through several more or less distinct phases: (a) the lure of big prospective dividends or gains in *income* in the remote future; (b) the hope of selling at a profit, and realizing a *capital* gain in the immediate future; (c) the vogue of reckless promotions, taking advantage of the habituation of the public to great expectations; (d) the development of downright fraud, imposing on a public which had grown credulous and gullible.

When it is too late the dupes discover scandals like the Hatry, Krueger, and Insull scandals. At least one book has been written to prove that crises are due to frauds of clever promoters. But probably these frauds could never have become so great without the original starters of real opportunities to invest lucratively. There is probably always a very real basis for the "new era" psychology before it runs away with its victims. This was certainly the case before 1929.

48. In summary, we find that: (1) economic changes include steady trends and unsteady occasional disturbances which act as starters for cyclical oscillations of innumerable kinds; (2) among the many occasional disturbances, are new opportunities to invest, especially because of new inventions; (3) these, with other causes, sometimes conspire to lead to a great volume of over-indebtedness; (4) this, in turn, leads to attempts to liquidate; (5) these, in turn, lead (*unless counteracted by reflation*) to falling prices or a swelling dollar; (6) the dollar may swell faster than the number of dollars owed shrinks; (7) in that case, liquidation does not really liquidate but actually aggravates the debts, and the depression grows worse instead of better, as indicated by all nine factors; (8) the ways out are either *via laissez faire* (bankruptcy) or scientific medication (reflation), and reflation might just as well have been applied in the first place.

49. The general correctness of the above "debt-deflation theory of great depressions" is, I believe, evidenced by experience in the present and previous great depressions. Future studies by others will doubtless check up on this opinion. One way is to compare different countries simultaneously. If the "debt-deflation theory" is correct, the infectiousness of depressions internationally is chiefly due to a common gold (or other) monetary standard and there should be found little tendency for a depression to pass from a deflating to an inflating, or stabilizing, country.

SOME NEW FEATURES

As stated at the outset, several features of the above analysis are, as far as I know, new. Some of these are too unimportant or self-evident to stress. The one (No. 32 above; also 36) which I do venture to stress most is the theory that when over-indebtedness is so great as to depress prices faster than liquidation, the mass effort to get out of debt sinks us more deeply into debt.[4] I would also like to emphasize the whole logical articulation of the nine factors, of which debt and deflation are the two chief (Nos. 23, 24, and 28, above). I would call attention to *new investment opportunities* as the important "starter" of over-indebtedness (Nos. 44, 45). Finally, I would emphasize the important corollary, of the debt-deflation theory, that great depressions are curable and preventable through reflation and stabilization (Nos. 38–42).

Yale University

[4] This interaction between liquidation and deflation did not occur to me until 1931, although, with others, I had since 1909 been stressing the fact that deflation tended toward depression and inflation toward a boom.

This debt-deflation theory was first stated in my lectures at Yale in 1931, and first stated publicly before the American Association for the Advancement of Science, on January 1, 1932. It is fully set forth in my *Booms and Depressions*, 1932, and some special features of my general views on cycle theory in "Business Cycles as Facts or Tendencies" in *Economische Opstellen Aangeboden aan Prof. C. A. Verrijn Stuart*, Haarlem, 1931. Certain sorts of disequilibrium are discussed in other writings. The rôle of the lag between real and nominal interest is discussed in *The Purchasing Power of Money*, Macmillan, New York, 1911; and more fully in *The Theory of Interest*, Macmillan, New York, 1930, as well as the effects of inequality of foresight. Some statistical verification will be found in "Our Unstable Dollar and the So-called Business Cycle," *Journal of the American Statistical Association*, June, 1925, pp. 179–202, and "The Relation of Employment to the Price Level" (address given before a section of the American Association for the Advancement of Science, Atlantic City, N. J., December 28, 1932, and later published in *Stabilization of Employment*, edited by Charles F. Roos, The Principia Press, Inc., Bloomingdale, Ind., 1933, pp. 152–159). See Charts I, II, III. Some statistical verification will be found in *The Stock Market Crash and After*, Macmillan, New York, 1930.

A selected bibliography of the writings of others is given in Appendix III of *Booms and Depressions*, Adelphi Company, New York, 1932. This bibliography omitted Veblen's *Theory of Business Enterprise*, Charles Scribner's Sons, New York, 1904, Chapter VII of which, Professor Wesley C. Mitchell points out, probably comes nearest to the debt-deflation theory. Hawtrey's writings seem the next nearest. Professor Alvin H. Hansen informs me that Professor Paxson, of the American History Department of the University of Wisconsin, in a course on the History of the West some twenty years ago, stressed the debt factor and its relation to deflation. But, so far as I know, no one hitherto has pointed out how debt liquidation defeats itself via deflation nor several other features of the present "creed." If any clear-cut anticipation exists, it can never have been prominently set forth, for even the word "debt" is missing in the indexes of the treatises on the subject.

CHARTS

The following eight charts are all on the "ratio scale" excepting Charts II, III, V, VI, and curve P' of Chart I. The particular ratio scale used is indicated in each case.

It will be noted that in Charts VII and VIII all curves have a common ratio scale, as indicated by the inset at the right in both charts, except "Brokers' Loans" in Chart VII and "Failures Numbers," "Failures Liabilities," and "Shares Traded" in Chart VIII, which four curves have another, "reduced" i.e., smaller, common scale, as indicated by the inset at the left of Chart VIII.

It will be further noted that "Money in Circulation," "Failures Numbers," and "Failures Liabilities" are inverted.

The full details of how $\overline{P'}$ in Charts II and III is derived from P' in Chart I and also how P' in Chart I is derived from P are given in "Our Unstable Dollar and the So-Called Business Cycle," *Journal of the American Statistical Association*, June, 1925.

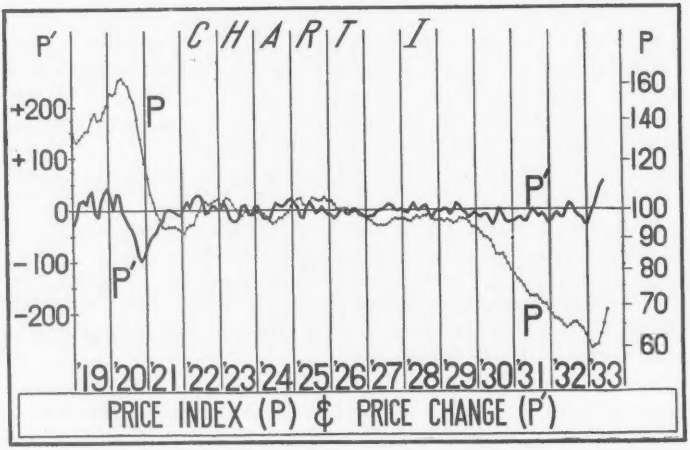


CHART I
P'
+200
+100
0
-100
-200
P
P'
P
P'
P
160
140
120
100
90
80
70
60
19 20 21 22 23 24 25 26 27 28 29 30 31 32 33
PRICE INDEX (P) & PRICE CHANGE (P')

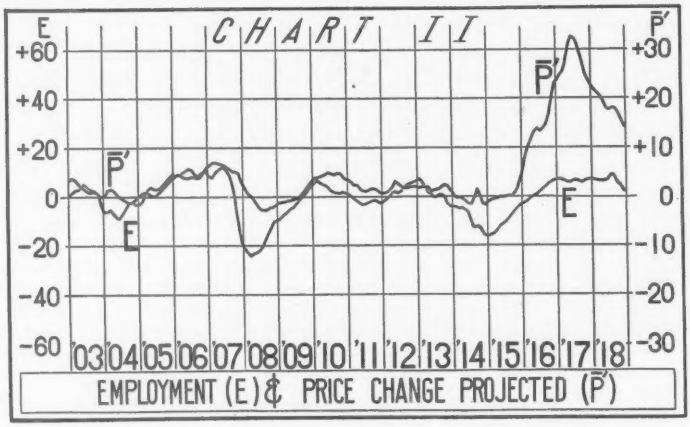


CHART II
E
+60
+40
+20
0
-20
-40
-60
P̄'
+30
+20
+10
0
-10
-20
-30
P̄'
P̄'
E
E
03 04 05 06 07 08 09 10 11 12 13 14 15 16 17 18
EMPLOYMENT (E) & PRICE CHANGE PROJECTED (P̄')

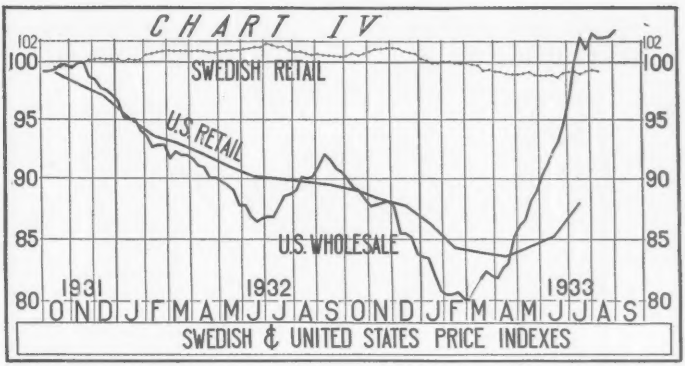
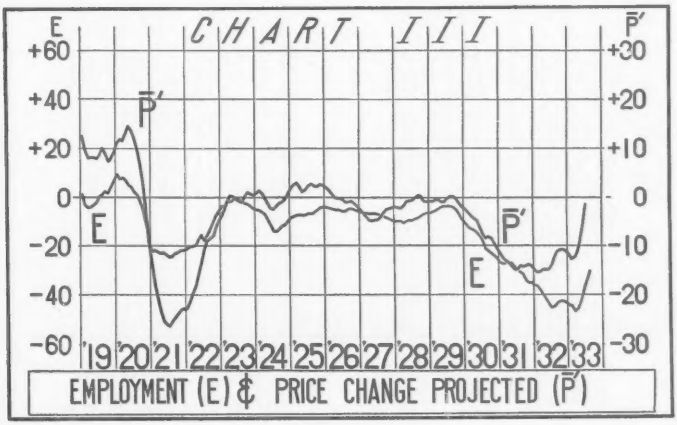
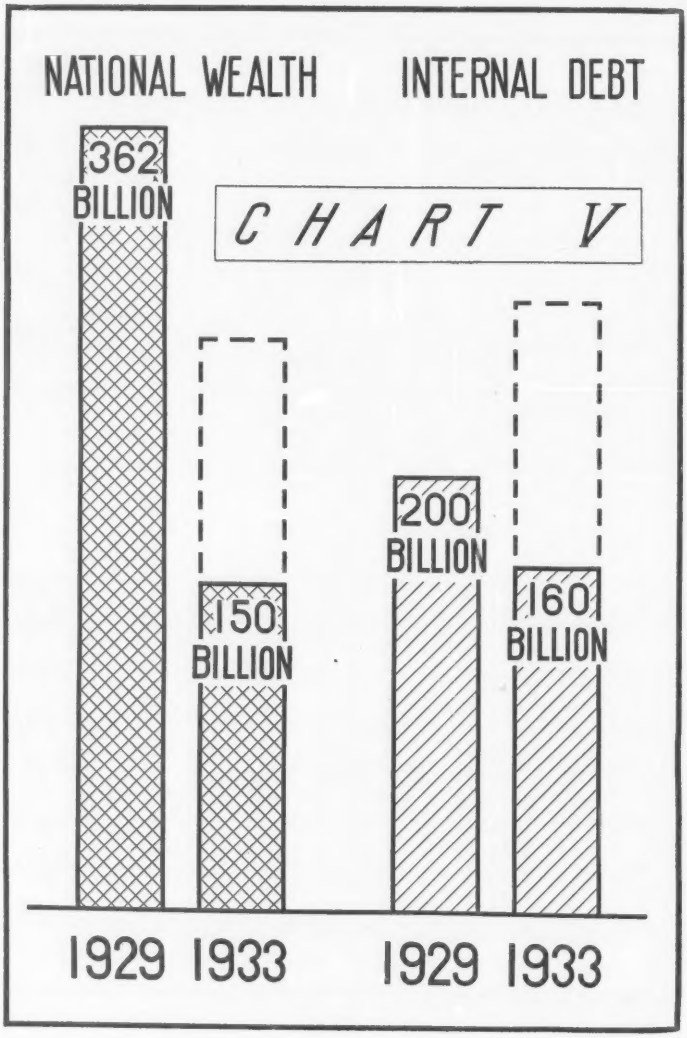
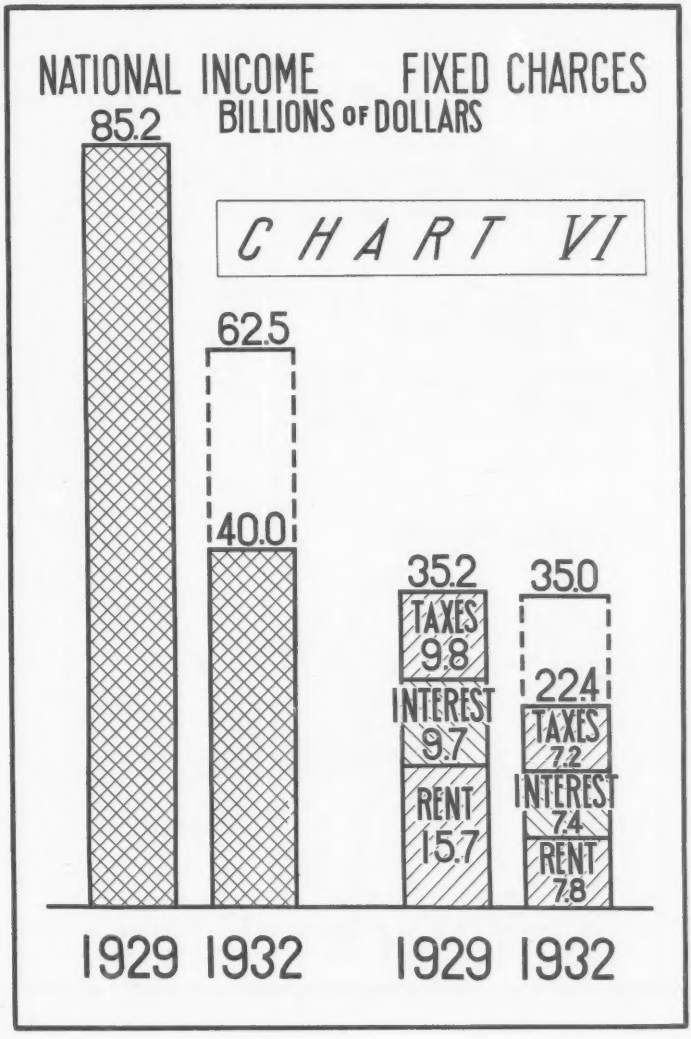


CHART IV
102
100
95
90
85
80
SWEDISH RETAIL
U.S. RETAIL
U.S. WHOLESALE
1931
1932
1933
O N D J F M A M J J A S O N D J F M A M J J A S
SWEDISH & UNITED STATES PRICE INDEXES


E
P̄′
CHART III
+60
+40
+20
0
-20
-40
-60
+30
+20
+10
0
-10
-20
-30
19 20 21 22 23 24 25 26 27 28 29 30 31 32 33
EMPLOYMENT (E) & PRICE CHANGE PROJECTED (P̄′)


NATIONAL WEALTH
INTERNAL DEBT
362 BILLION
CHART V
150 BILLION
200 BILLION
160 BILLION
1929
1933
1929
1933


NATIONAL INCOME
FIXED CHARGES
BILLIONS OF DOLLARS
85.2
CHART VI
62.5
40.0
35.2
TAXES
9.8
INTEREST
9.7
RENT
15.7
35.0
22.4
TAXES
7.2
INTEREST
7.4
RENT
7.8
1929
1932
1929
1932

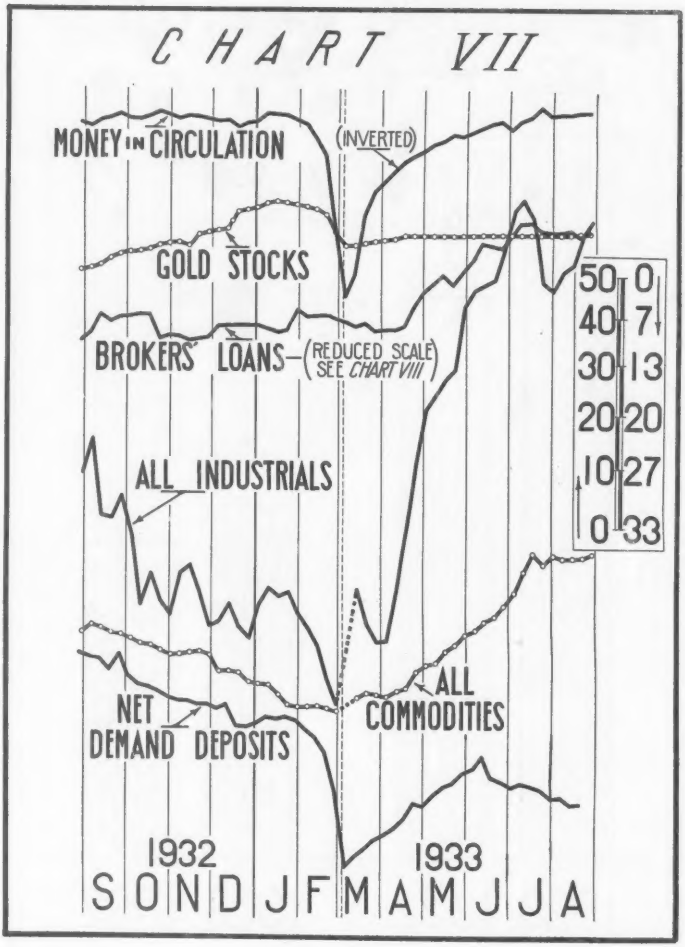


CHART VII
MONEY IN CIRCULATION
(INVERTED)
GOLD STOCKS
BROKERS' LOANS—(REDUCED SCALE SEE CHART VIII)
ALL INDUSTRIALS
NET DEMAND DEPOSITS
ALL COMMODITIES
50 0
40 7
30 13
20 20
10 27
0 33
1932
1933
S O N D J F M A M J J A

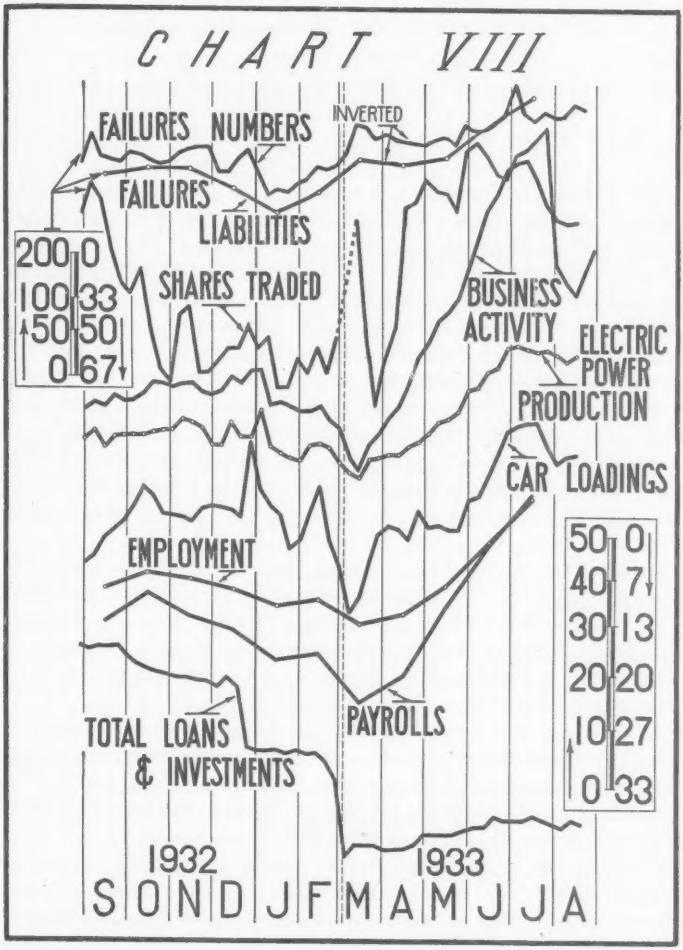


CHART VIII
FAILURES NUMBERS
INVERTED
FAILURES
LIABILITIES
200 0
100 33
50 50
0 67
SHARES TRADED
BUSINESS ACTIVITY
ELECTRIC POWER PRODUCTION
CAR LOADINGS
50 0
40 7
30 13
20 20
10 27
0 33
EMPLOYMENT
TOTAL LOANS & INVESTMENTS
PAYROLLS
1932
1933
S O N D J F M A M J J A

THE ACTION OF ECONOMIC FORCES IN PRODUCING FREQUENCY DISTRIBUTIONS OF INCOME, PRICES, AND OTHER PHENOMENA: A SUGGESTION FOR STUDY

By A. L. Bowley

Paper read before the Meeting of the Econometric Society, Paris, October, 1932

There has been, to some extent, a divergence of purpose between the work of mathematical statisticians and mathematical economists. The former have studied variation, such as the form and measurement of frequency groups, and the correlation between two or more variables; the latter have studied equations of equilibrium, in which the data represent totals, margins, the representative, the average—single quantities in general—where heterogeneity does not enter into the argument. Econometricians have, indeed, worked on variation and covariation, but most attention has been given to examples where time is one of the variables, that is to time series; and the relationships obtained have been in the form of a demand curve in one group of studies, to periodic curves in time in another, and to correlation between quantities which are paired in time, but not variable at a given instant of time.

I wish to direct attention to a field which has not been thoroughly explored, and in which, so far as I know, little work is being done. What are the forces which produce the variation found in common phenomena, how far are they economic, what changes in frequency distributions are produced by economic forces, how can they best be described or measured?

To name one example, which has already been studied: Incomes in certain circumstances are distributed in accordance with Pareto's equation. What has produced this particular form; is the form modified by economic change, or rather by changes in the sociological or legal spheres; how can the effects of these best be measured?

Variation has been studied in many fields and it may be well to mark off those which are not primarily economic from those which are. We will exclude anthropometry and biometry, and statistics of mortality and morbidity. Demographic statistics generally would enter only in their economic aspects, such as age distribution in occupations, and concentration of population. Within the economic sphere proper, there are many distributions which have been the subject of careful work: price changes in time, local variations in price, prices of securities, size of industrial firms, rates of profit and dividends, ratio of

profits to cost, distribution of income as already instanced, distribution of salaries, of earnings, of wages within a population, local variations of wages, variation of expenditure in family budgets. No doubt the list could be greatly prolonged.

In all these cases statistical analysis reveals a form or pattern. Sometimes close relationship to a definite mathematical form is found; sometimes the description is by averages, moments, percentiles. In the former case there are such parameters as determine Pareto's equation for incomes or the Makeham-Gompertz law for the Table of Survivals, or the Law of Error. In the latter there are appropriate measurements of averages, deviations, asymmetry, kurtosis. These parameters, or other measurements, vary from place to place, and from time to time. Investigators have generally been content to establish the existence of the mathematical form, if any, and to record the values. That is to say the work has been primarily descriptive.

Now these forms, whether expressible by a simple function or not, have doubtless been reached by the action of economic, social, physical, or psychological forces in a given or changing environment, and changes in their measurements are being produced by similar forces. We can hardly hope to obtain a complete explanation even in the simplest case, but by the consideration of the circumstances, and by comparisons in time and place, it may be possible to isolate groups of forces, to disentangle those which are economic from those which are physical or sociological; and so to get some insight into the nature and cause of variation, and ultimately to identify the change in values with definite economic movements.

Let us first approach the problem from the static viewpoint, and consider the nature and possible generation of existing frequency distributions. The question at which I have worked is the distribution of incomes, and discussion of this will at least illustrate a method of approach.

It is known that many anthropometrical measurements are distributed in accordance with the Gaussian Normal Law of Error. Galton formed the opinion or hypothesis, on rather indefinite evidence, that mental and other non-physical attributes were similarly distributed. It has been assumed too readily that this theorem was established, and could be applied to non-measurable attributes. It has, however, received considerable support from the actual distribution of such measurements as examination marks, with the interesting result that it is the moderately asymmetrical variant of the Normal Law that is reached. We may then start with the working hypothesis that aptitudes, so far as they are measurable, are normally, or sub-normally, distributed.

If, then, persons all had the same education, environment, and opportunity, were paid by piece-rates, and worked with equal zeal, their earnings in a stated time would be normally distributed, with possible gaps at the extremities if the least efficient were not employed and the most efficient had passed on to a higher type of work. Such conditions are approximately illustrated in cotton-weaving in Lancashire, where tradition, environment, education, and opportunity for work, are approximately homogeneous, and payment is by piece. In fact, records of earnings show at least a tendency to this normality.

If we take a field of work wider in occupation and locality, we should expect a combination of normal curves with different centres and standard deviations. If these curves were numerous, and certain other conditions were satisfied, they would, when combined, tend to a normal distribution, which would probably be more unsymmetrical than the originals.

There is, however, an important modification in this process when we consider the actual determination of wages. A very considerable proportion of wages in England is on a strictly time basis, and a further large part is on a mixed time and piece basis. Pure piece-rates are found only in a not very large minority of occupations. Further, there are legal minimum rates in some industries. Time-rates of wages, arranged by bargaining between employers' and workmen's associations, are popularly supposed to be uniform for all in an occupation and district. In fact, these rates are frequently only minimum rates, and actual earnings vary to some extent from them, especially if there is a piece-bonus or similar addition to payments, or if there are different grades of labour. There is, however, a strong tendency towards equality of payment, and the relation of wages to natural aptitude in a group of industries would be shown, if at all, by a variation from one occupation to another. Our combined frequency group would not be continuous but would consist of a group of concentrated curves whose centres were different, e.g. a curve for unskilled labour, for skilled labour, etc.

There are many other considerations, such as different environments, education, and opportunities, which would separate wage distribution from natural aptitude. It may, perhaps, be held that in a community in which resources were used to their best advantage, persons, would, at maturity, arrive at the work for which they were best fitted, and if the net efficiency earnings were equal—as may be expected in certain conditions—these earnings would again be normally distributed.

The line of thought I wish to pursue, however, is more mathematical. In fact, a medley of curves massed together gives a normal appearance even in apparently unfavorable circumstances. If normality is found

in the wages of a large and heterogeneous community, this may not be because careers are open to all, and earnings are proportional to aptitude, but because the numerous obstacles to freedom of opportunity and bargaining and the levelling of wages within occupations make that kind of complex which results in normality from a number of abnormal factors.

If this at all accurately describes the conditions in a country, we can (if the statistics are sufficient) study the detailed curves for particular occupations and districts, and watch how they shape when they are combined. I have some material of this kind (at present unpublished) that has accumulated in the *New Survey of London Life and Labor* which is resulting in a series of Volumes, of which that published in the autumn of 1932 contains some of the relevant data.

1. Weekly time wages in all occupations paid on a time basis in East London. (a) Adult males.—The great majority of these wages are based on standard rates agreed by the employers and workmen. The data were obtained from direct enquiries of wages received in a normal week in 12,000 households, which formed a representative example.

The average wage is approximately 63*s*. The standard deviation 15*s*. There is slight negative asymmetry.

The distribution is fairly continuous and there is a strong tendency to normality, disturbed by a moderate heaping in the grade 57*s*.6*d*. to 62*s*.6*d*., which contains the most usual rate for unskilled labour, and an unexplained heaping about 45*s*.

TABLE I

EAST LONDON ADULT MALE TIME WAGES PER NORMAL WEEK PER 1000

Wages	Normal Curve	Actual
Under 34*s*.	25	30
34*s*. to 37*s*.6*d*.	21	13
37*s*.6*d*. " 42*s*.6*d*.	31	34
42*s*.6*d*. " 47*s*.6*d*	77	40
47*s*.6*d*. " 52*s*.6*d*.	91	106
52*s*.6*d*. " 57*s*.6*d*.	114	125
57*s*.6*d*. " 62*s*.6*d*.	133	176
62*s*.6*d*. " 67*s*.6*d*.	130	126
67*s*.6*d*. " 72*s*.6*d*.	117	110
72*s*.6*d*. " 77*s*.6*d*.	97	69
77*s*.6*d*. " 82*s*.6*d*.	69	87
82*s*.6*d*. " 87*s*.6*d*.	44	31
87*s*.6*d*. " 92*s*.6*d*.	27	27
92*s*.6*d*. " 97*s*.6*d*.	14	8
97*s*.6*d*. "102*s*.6*d*.	6	11
102*s*.6*d*. or more	4	7
	1,000	1,000

The departure from normality is considerably greater than would be obtained if actually the wages of the 9500 men on which the table was based were independent of each other and normally distributed. Professor Pearson's test would give $\chi^2 = 700$ approx., $n = 16$, and P infinitesimal.

Normality has been nearly reached, but by combination of groups internally non-independent.

(b) Adult women.—It is a curious fact that in a tabulation of adult women's wages in one borough (Stepney) normality is reached. The occupations are very varied—domestic service, dressmaking, factory work of various kinds, shop-assistants, and typists (all coming from working-class families). All methods of payment are included.

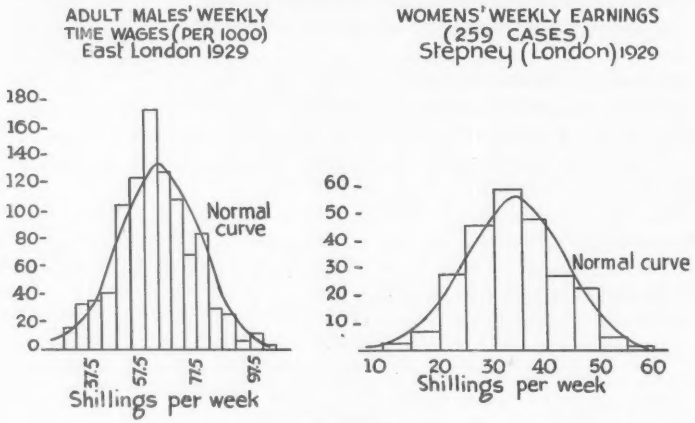


CHART I

In East Ham, on the other hand, the grouping is not regular, though continuity is hardly to be expected with only 102 cases. It is, in fact, disturbed by the inclusion of typists, etc., a class not well represented in Stepney. It appears then that some, at least, of the districts are not large enough nor heterogeneous enough for the separate wage-groups to merge into normality.

If married women's earnings were also included, we should still get a close approximation to normality in Stepney, though the nature and average wage of married women's occupations differ from those for single women.

The analysis has illustrated how a normal distribution may arise either from a number of independent observations, or from the aggregation of a number of concentrated groups. The Pearsonian test of fit

may indicate which system is present. Where his P is small there is not independence, even if there is an apparent approximation to normality. But the argument is not sufficiently firm to allow us to trace with certainty the influence of monopoly or control on the distribution of wages.

TABLE II
UNMARRIED WOMEN'S EARNINGS IN A NORMAL WEEK (ACTUAL NUMBERS)

	Stepney			East Ham	
	Normal curve u	In sample v	$(v-u)^2/u$	Normal u	Sample v
Under 10*s.*	1	1	0	0	2
10*s.* to 15*s.*	4	3	.25	1	
15*s.* " 20*s.*	11½	7	1.76	4	4
20*s.* " 25*s.*	25	29	.64	6	7
25*s.* " 30*s.*	42½	47	.48	22	12
30*s.* " 35*s.*	54	60	.67	12	16
35*s.* " 40*s.*	51½	48	.24	13	17
40*s.* " 45*s.*	37	28	2.19	14	16
45*s.* " 50*s.*	20½	24	.60	7	13
50*s.* " 55*s.*	8½	6	.75	17	8
55*s.* " 60*s.*	2½	3	.10	3	4
60*s.* or more	1	3	4.00	3	3
	259	259	11.68	102	102
	$\chi^2=11.7$ $n=12$ $P=.4$ approx. Average 34*s.*3*d.* Standard deviation 9.25*s.*			$\chi^2=19$, $n=11$, $P=.04$ approx. Average 37*s.*6*d.* Standard deviation 11.2*s.*	

So far this is a static view of wages at one time and place. A series of such investigations would indicate the influence of changing circumstances by changes of the average, of the standard deviation, and of the measurement of consistency with normality.

2. Family Income.—We obtain a different form, however, if we take a further step and study family income. The number of workers per family varies thus:

Number of Earners	Per 1,000 families
0	79
1	540
2	185
3	108
4	55
5 or more	34
	1,000

and in addition to earnings there are in some cases pensions or other sources of income.

Thus the family income may be written $I=p+aw_1+bw_2+cw_3$, where a is the number of men earning, b the number of women, c the number of children, w_1, w_2, w_3 are the wages and p the income not from wages. All these quantities have their special frequency curves; that of a is markedly skew since it has the value 1 in the majority of cases, while b is usually 0 and seldom greater than 1. Consideration of these facts will lead us to expect a curve disturbed from normality, in a definite way.

Neither the normal curves nor Pareto's formula, in their simplest form, give even a plausible picture of the variations, but each of these curves can be so modified as to fit the figures.

For the normal curve I use Edgeworth's little tried "method of translation." Here it is assumed that the quantity observed x is related by a parabolic equation to another quantity which has normal frequency.

The equation used for the Table is

$$x=26.7\ (\xi+.307\xi^2+.29\xi^3),$$

where the frequency curve for ξ is $y=1/\sqrt{2\pi}\cdot e^{-\xi^2/2}$. The coefficient 26.7 is related to the standard deviation, the other numbers to the third and fourth moments. This curve gives too many cases at the extreme upper end of the scale, while if the constants are changed to fit this, there is a deficit in the neighbourhood of the mode. The agreement is fairly close throughout.

When the logarithms of the incomes and of the numbers in receipt of more than a given income, are graphed, we do not get a straight line as in Pareto's formula, but marked curvature.

If z is written for the number of incomes above x, and $\zeta=\log x$, the equation $z=-3.00+7.844\zeta-2.579\zeta^2$ gives a good fit from 82.5*s*. upwards.

The figures are given in Table III.

The method of translation involves the median and three other constants. In the above it was obtained by equating the percentiles at the median, 42.5*s*., 92.5*s*., and 162.5*s*. Choices of other percentiles would, of course, modify the curve, but not, I think, so as to give a closer general agreement.

The modified form of Pareto's curve involves three constants only. In the above the percentiles at 42.5*s*., 112.5*s*. and 192.5*s*. were approximately equated.

We can, of course, force agreement at a certain number of points from any equation. Graphic work is often sufficient to suggest what

points should be chosen.[1] The diagram annexed indicates that neither formula is of a nature to exhibit all the variations. Other equations might serve better, but some want of continuity in the observations is suggested.

TABLE III
FAMILY INCOMES PER WEEK IN EAST LONDON
Per 1,000

Range of incomes	Observed	Calculated	
		Method of translation	Pareto's formula modified
202.5s or more	22	30	24
192.5 to 202.5	7	5	4
182.5 " 192.5	7	7	7
172.5 " 182.5	10	8	9
162.5 " 172.5	11	9	11
152.5 " 162.5	17	12	14
142.5 " 152.5	18	15	16
132.5 " 142.5	22	20	23
122.5 " 132.5	28	25	28
112.5 " 122.5	32	33	37
102.5 " 112.5	43	46	48
92.5 " 102.5	54	61	62
82.5 " 92.5	81	88	80
72.5 " 82.5	124	125	101
62.5 " 72.5	158	158	125
52.5 " 62.5	156	135	139
42.5 " 52.5	75	87	142
34.0 " 42.5	36	54	74
Under 34.0	99	82	56
	1,000	1,000	1,000

It has long been known that at the lower end of the income scale, where weekly wages are the main constituent, the distribution has affinity to normality, and at the upper end of high salaries and large incomes from property, the Pareto form is appropriate. The illustration given shows that there is a possible transition from one of the forms to the other—over the range 82.5s. to 200s. the curves are nearly identical. In the higher ranges the coefficient of ζ^2 is smaller, and though it is hardly possible that one form can describe the whole range, yet a family of curves whose constants are gradually modified might serve.

[1] Mathematical tests of closest fit are not appropriate when it is evident from the graph that there is aberration at the extremes. It may be noted that the Lorenz curve applied to Pareto's form and indeed the original Pareto's equation, give an integral curve, not a frequency curve, and from them it is extremely difficult to judge how close is the agreement between the data and the equation. The diagram annexed exaggerates the closeness of the agreement.

3. Commodity prices.—A considerable amount of investigation has already been devoted to the "behaviour of prices," but the subject is so important that it is worth while to study it further with new statistics from the point of view of this paper.

Is the distribution of prices (taken as percentages of their amount in a chosen base year) consistent with the complex of causation which

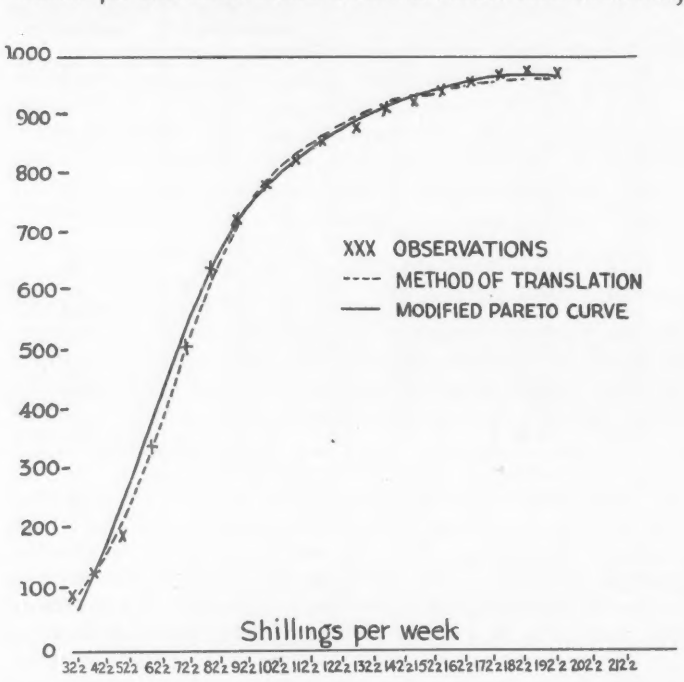


CHART II

gives rise to normal distribution? For example, consider the 45 percentages which lead to the *Statist* index-number of wholesale prices.

So far as I know, there is no unique test of conformity to normal distribution in the case of a small number of discrete scale measurements. I have applied the method of placing side by side the percentiles (deciles, sextiles) of the actual distribution and those of a normal distribution which has the same range between two selected percentiles.

In this case the first and ninth deciles are taken as pivotal numbers. The results for certain years are shown in Table IV, which is a corrected extension of figures given in the *Economic Journal*, 1928, p. 218.

TABLE IV

STATIST INDEX. DISTRIBUTION OF 45 WHOLESALE PRICE-PERCENTAGES

A—actual distribution. B—normal distribution equated at D_1 and D_9.

Year	1926		1920		1925		1929		1931		1931	
Base	1867–77		1913		1913		1913		1924		1929	
Deciles	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.
D_1	77=	77	176=	176	117=	117	104=	104	36=	36	48=	48
D_2	93½	93	204½	236	138	131	113	115	41½	46	52½	57½
D_3	98	104	220	279	148	141	126	123	47	52	58	63
D_4	106½	114	254½	316	153½	149	130	129	56	57½	61½	68
D_5	121	123½	299	351	163	157	136	135½	62	63	71	73
D_6	137½	133	310½	386	169	165	144	142	66½	68½	74½	78
D_7	146	142½	343	423	176	173	150	148	70	74	85	83
D_8	158	154	377	466	190½	183	153½	156	79½	80	88½	89½
D_9	170=	170	526=	526	197=	197	167=	167	90=	90	98=	98
$V=\frac{D_7-D_3}{D_7+D_3}$	.20		.22		.09		.09		.20		.12	

The agreement between the distribution of the observations and normal distribution is closest when we take the period 1913 to 1929. But it is as close as we should expect from only 45 cases over the long period 1867–77 to 1926. There is distortion from normality especially when we take the year of inflation (1920), which is, in fact, only slightly corrected if we make comparison on the logarithmic scale. There is also distortion on the lower half of the group when we take 1931, a year of falling prices. Thus it appears that we obtain normal distribution in long periods, or in years not marked by economic disequilibrium, while there are aberrations (which may on further analysis be found to be systematic) when prices are changing rapidly, and in times of crisis.

The relationship between wholesale and retail prices may be expected to be approximately of the form $p=k\,P+lw$, where p and P are retail and wholesale prices, w is wages, and k is constant, and l is constant for all prices in one year but may vary from year to year. During the period in the table below from 1924 to 1932, there was little variation of money wages in England. Hence the distribution of retail prices may be expected to differ from that of wholesale prices only in centre and scale, and not in form.

In a study of British retail food prices compared with relevant wholesale prices, I found the equation $p=.77P+23$ where P was dated

two months before p, and both applied to weighted averages (*Lloyds Bank, Monthly Review*, June, 1930).

So far as I know there have been no published analyses in which the form of the variability of retail prices is compared with that of wholesale, and I have not pursued this question far. A convenient first measurement of form may be derived from the table above, viz.

$$(D_7 - D_3) \div (D_7 + D_3) = V.$$

This has been computed also from the data of 21 retail food prices in Great Britain, which form the subject of the subsequent paragraphs. The foods contained in the two lists are not the same; the 45 wholesale commodities do not in fact include all of the 21 retail commodities.

TABLE V
VALUES OF V

Wholesale prices (year's average.)									
Year	1926	1920	1925	1929	1931	1931	—	—	—
Base	1867–77	1913	1913	1913	1924	1929	—	—	—
V	.20	.22	.09	.09	.20	.12	—	—	—

Retail prices (month of August)									
Year	—	—	1925	1929	1931	1931	1932	1932	1924
Base	—	—	1914	1914	1924	1929	1924	1929	1914
—	—	—	.09	.07	.09	.08	.04	.06	.08

The resemblances and differences in these quantities evidently deserve study which time does not now permit.

I return to the main question, i.e., whether retail price changes are normally distributed.

The 21 food prices used in the computation of the British Cost of Living Index number are not random nor uncorrelated, and do not cover a wide range of food. Nevertheless, they show the same kind of relationship to the normal curve as do wholesale prices, with variation in particular years. Exact comparison is not possible, because 1914 must be taken for retail and 1913 for wholesale prices, but the movement between the average of 1913 and July 1914 was not important. In 1925 the distribution of retail price changes (base 1914) was less normal than of wholesale. When 1929 is compared with 1913 or 1914, normality is reached in both groups, as it is when 1929 is compared with 1924. There are some disturbances when 1931 is compared with 1924 or with 1929 in both groups. But it is remarkable that when 1932 is compared with 1924 or with 1929, the agreement is as close as could be expected with so few numbers.

Since the number of retail prices is so small, sextiles are used instead of deciles, and S_1 and S_5 are taken as fixed positions from which the other sextiles are computed from the normal table.

TABLE VI
DISTRIBUTION OF 21 RETAIL FOOD PRICES. AUGUST IN EACH YEAR[1]

Year	1924		1925		1929		1932	
Base	1914		1914		1914		1914	
	N	A	N	A	N	A	N	A
S_1	138	138	136	136	133	133	98	98
S_2	150½	145½	150	155	143	142½	112½	109
S_3	160½	150	161½	161	151	150	124	117
S_4	170½	164½	173	181	159	159	135½	125
S_5	193	193	187	187	169	169	150	150

Year	1931		1932		1931		1932	
Base	1924		1924		1929		1929	
	N	A	N	A	N	A	N	A
S_1	72	72	67	67	73	73	73	73
S_2	78½	78	74	76	79	82	78	78
S_3	83½	83	79½	79	84	89	82	83
S_4	88½	87½	85	82	89	92½	86	86½
S_5	95	95	92	92	95	95	91	91

S_3 is the median.

[1] Actually approximately August 1 in all years except 1914, in which July was taken.

4. Security prices.—The last numerical example I propose to take is of the prices of securities (stocks). It is known that in the construction of an index number of security prices there are technical differences from that of an index of prices, and I had hoped that I should find a different form of frequency distribution. But in fact I am brought back to the normal curve. I use the numbers on which the index of the London and Cambridge Economic Service is computed.

In the period of rapid fall (22 per cent on the unweighted average) from July, 1929, to July, 1930, the 90 prices concerned are distributed normally, as tested by the Pearsonian method.

There is some displacement from normality from below to above the average, but not more than might happen in random selection. In Pearson's test $X^2 = 11.5$, $P = .26$ approx.

But when we take the longer period 1924 to 1930, resemblance to the normal curve is not present. The extreme numbers are now 6 and 2033, and the figures suggest that a logarithmic scale would be appropriate.

TABLE VII
NINETY SECURITY PRICES. JULY, 1930, AS PER CENT OF JULY, 1929

Equated at D_2 and D_8			Average 78.1, $\sigma = 19.5$		
Deciles and Quartiles	Actual	Normal	Range	Number of cases	
				Actual	Normal
D_1	51	57	25–	1	1.6
D_2	65 =	65	35–	3	3.4
Q_1	67	68	45–	7½	7.6
D_3	72	71	55–	7	13.1
D_4	79½	76	65–	13	17.4
D_5	84	80½	75–	17	17.9
D_6	87½	85	85–	22½	14.2
D_7	89½	90	95–	12½	8.7
Q_3	93	93	105–	4	4.1
D_8	96 =	96	115–	1½	1.5
D_9	104	104	125–	0	0.4
			135–	1	0.1
				90	90

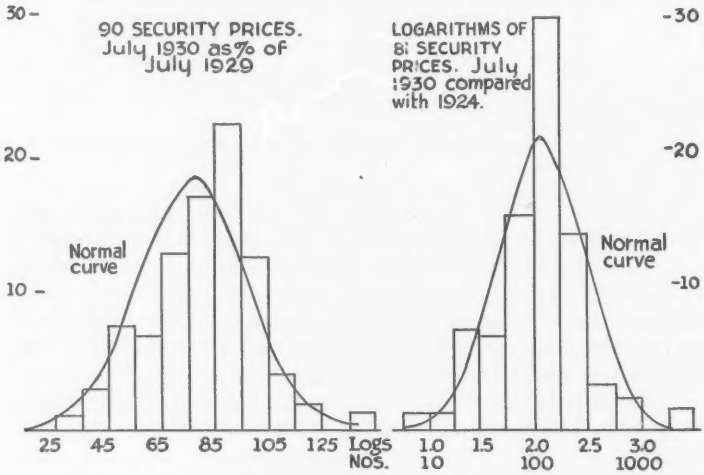


CHART III

The test of fit cannot be exactly applied and in fact the extreme cases are beyond expectation. However, the curve is approximately normal, and we have that, in a period of expansion, the logarithms of the price percentages tend to normal distribution, and, in depression, the price percentages themselves.

TABLE VIII
EIGHTY-ONE SECURITY PRICES

July, 1930, as per cent of the year 1924

Deciles and quartiles	Normal natural		Normal logarithmic		Actual
D_1	24		45½		28
D_2	62	=	62	=	62
Q_1	77		70		70
D_3	90		78		78
D_4	113		95		102
D_5	135		114		120
D_6	157		136		147
D_7	180½		166		160
Q_2	193½		185		179
D_8	208	=	208	=	208
D_9	246		285		273
Equation is made at D_2 and D_8					Geomtric mean 112

Logarithms of July 1930 as per cent of the year 1924
Average 2.04; $\sigma = .387$

Range of logarithm	Number of Cases	
	Actual	Calculated
Below 1.00	2	1.6
1.25 to 1.50	7	4.9
1.50 " 1.75	6½	11.7
1.75 " 2.00	15½	18.8
2.00 " 2.25	30	20.1
2.25 " 2.50	14	14.3
2.50 " 2.75	3	6.8
2.75 " 3.00	2	2.2
3.00 and upward	1	0.6
	81	81.0

$n = 9$, $\chi^2 = 11.2$, $P. = .19$ approx.

A rougher computation, confined to the period of expansion, for the comparison of July, 1929 with 1924, gives similar results. Average 2.14, $\sigma = .347$.

The fit is obviously close, except for a displacement above 2.70 which is log 500.

I had not intended when I planned this paper to confine it to cases when the normal curve of error is applicable. But, in fact, most of the material on which I have worked is of a similar nature—that is, one obtains a fair approximation to the normal curve or one of its simpler modifications with some discontinuity and with a very small propor-

TABLE IX

Range of logarithm	Number of cases	
	Actual	Calculated
Below 1.30	1	0.6
1.30 to 1.50	1	2.0
1.50 " 1.70	6	5.6
1.70 " 1.90	8	11.4
1.90 " 2.10	21	16.7
2.10 " 2.30	19	17.9
2.30 " 2.50	17	13.8
2.50 " 2.70	3	7.7
2.70 " 2.90	0	3.1
2.90 " 3.10	2	0.9
3.10 and upwards	2	0.3
	80	80

tion of cases that lie beyond the ordinary range—giants or dwarfs in a homogeneous population. Examples of this kind not here included are the distribution of expenditure on food per adult male in a collection of budgets (*Statistical Journal*, 1919, p. 359), and the distribution of the densities of populations of rural districts in Great Britain (*Statistical Journal*, 1914, pp. 634–644).

The following conclusions are suggested:

1. The distribution of measurements of economic phenomena manifested by a reasonably homogeneous group at one time has the continuity of an algebraic function or law.

2. Where the phenomena are mutually independent, or are an aggregate of numerous smaller groups, there is a tendency to normality, which can be expressed by the average, the standard deviation, and (if necessary) one or two other parameters.

3. In other cases, other algebraic functions may be found, or at least the distribution can be described by a limited number of percentiles.

4. Steady economic progress may be expected to affect the average, standard deviation, and other parameters of the group.

5. Economic disequilibrium is shown by distortion of the curve.

6. Departure from the normality that results from independence, is an indication of the presence of monopoly or control.

7. Suitable hypotheses of economic action and natural variation may be tested by the distributions found.

8. The same form of distribution may result from different systems of causation, and it is not safe to argue from the form to the system.

London School of Economics

ANNUAL SURVEY OF STATISTICAL INFORMATION:

THE BRANCHES OF NATIONAL SPENDING

By Jakob Marschak

I. THE ECONOMETRIC APPROACH TO STATISTICS

The econometric approach to statistics is a rather ambitious one. The econometrician is not content with mere description and historical or geographical comparison of economic data. He aims at obtaining invariant features of economic phenomena, invariant, at least, as long as the main institutional conditions of a given economic order do not substantially vary. He considers economic invariants as an aim worthy of scientific endeavor, even if they prove to be confined to some very general characteristics common to a loose "family of curves." Of course, the numerical formulation of such is in itself again nothing more than a description of facts. But in the econometric approach an endeavor is made to reduce these facts to rules which can stand the test of applicability to, or of forecasting of, *other* facts.

Among recent statistical works having immediate econometric interest, three more important groups may be discerned: (1) attempts towards a construction of demand and supply curves of certain commodities (including productive factors), (2) analysis of business cycles, and (3) numerical investigation into the branches of national spending, including saving or investment, and production (possibly connected with research into national stocks and cash), and into time changes in their proportions. Obviously, the three subjects are closely connected with one another. The proportions between the main economic currents depend on price ratios between commodities and/or productive factors; and the analysis of business cycles, if it is more than an enumeration of symptoms, is hardly possible without knowledge of the reactions to which the different commodities and productive factors—and consequently the different streams of production and spending—are likely to be originated by given changes of the circumstances. The three groups of research have, however, some differences in the technical apparatus employed, and these permit a separate review. We are concerned, in this review, with the third group only. On questions recently discussed in economic theory, such as the influence of profits or of capital accumulation on national productivity, some light may be thrown if we know the order of magnitude of the parameters concerned, and the intensity and velocity of their mutual reactions.

II. THE ESSENCE OF TABLEAUX ECONOMIQUES

We may represent all exchanges performed during a given period of time between the individuals $a, b, c, \cdots$ within a closed society by the following table.

Purchased by \ Sold by	a	b	c	—
a	aa	ab	ac	—
b	ba	bb	bc	—
c	ca	cb	cc	—
—	—	—	—	—

ab is the amount sold by a to b, expressed in value units, etc. The diagonal elements aa, bb, cc, may be considered as the amount consumed, by an individual, of commodities produced by himself. Any row and column having a common diagonal element, i.e., all purchases and all sales of the same individual, must result in equal sums, if we except the possibility of savings without investments or investments without savings. These sums (turnovers) obviously depend not only on the incomes of the individuals concerned, but also on the degree of the vertical concentration of particular industries. The influence of the latter—the "double reckoning" of materials bought by one producer from another—can be eliminated if we consider not all sales and purchases but only those occurring to "original productive services" (labor, capital, entrepreneurial services) on the one hand, and consumption goods and new investment goods (as distinct from those investment goods used for repairing and replacing old ones) on the other. The columns would then represent incomes, the rows the final spending. Hence the well-known double method of calculating national income either as the sum of all incomes or of all final spendings.

If, furthermore, (1) a row, or (2) a corresponding column, or (3) both, be expressed in physical instead of money units, the equality of both sums would disappear, and the movement of the ratio of these sums to each other would disclose (1) the movement of the money-prices of final commodities, usually bought by the individual, or (2) the movement of the money-prices of the productive factors supplied by the individual, or (3) the movement of the real exchange-relation between the final commodities and the productive factor under consideration.

Available statistics permit of summing up the sales of productive factors as well as the sales of final products (including new investment goods). The former can be gathered from the figures of "net output"

("net value added") given for some main branches by Censuses of Production, or can be ascertained from income statistics as supplied by fiscal revenue records, social insurance material, wage inquiries, etc. As to the stream of final products, retail trade statistics and family budgets would supply the bulk of information. These different ways can be mutually controlled and supplemented, as far as the total income is to be arrived at. The movements of the total income are, as D. H. Robertson has pointed out, the main thing to be considered in every description of business cycle. But the proportions of income assigned to different groups of producers or to different departments of spending, and the price relations expressed by them, are of no less importance; hence the question of rational grouping.

The reader will easily recognize in our table the "*Tableau Économique*" of Quesnay, or Marx's tables of Reproduction, if we substitute groups for individuals. In the case of Quesnay these groups are: (*a*) agriculture, (*b*) manufacture, (*c*) government. Accordingly, *ab* would be the agricultural product bought by towns, *ba* the manufactured product sold to villages, *aa* the agricultural product consumed in village, *ca* the farmers' taxes (the government services being suitably interpreted), etc. From the point of view of modern economic theory the only peculiarity of agricultural products consists, however, merely in their higher instability of supply; the division of Quesnay has, then, lost much of its significance.

The groups relevant to Marx's analysis are, on the other hand: (*a*) production of (new *and* replacing) producers' goods, (*b*) production of goods for workers' consumption, (*c*) production of goods for capitalists' personal consumption. We have here two principles of classification: a distinction between the original factors of production, and a distinction between goods according as they lie nearer to or farther from the consumption act. The latter principle was still more stressed by Menger and Böhm-Bawerk, who divided the process of production not into two (as Marx did) but into many "stages." Whether we distinguish each stage by the number of changes in property rights to be performed until the product reaches the consumer, or simply measure the time-distance separating the given production activity from the consumption act, in each case we obtain data of paramount importance for general and monetary theory.[1] With the second mode of measurement, moreover, a link between the tableau of "flows" and that of "stocks" would be established. The division of the final spending with consumption and investment can be easily combined with it.

[1] An attempt at this has been made in my article "Econometric Parameters in a Stationary Society with Monetary Circulation," to appear in ECONOMETRICA.

If the groups selected were (1) non-competing with one another, and (2) capable of infinitely quick reactions, the changes of proportions caused by given changes of outside circumstances could be attributed to variations of the single supply and demand functions, on the lines of the pure theory of international trade. Two further factors complicate this aim: (1) The groups of producers are, in different degrees, interchangeable with one another, and so are the groups of final goods; (2) the reactions of output as well as of prices, and, consequently, the reactions of the group-exchange ratios take time, and each a different time. We should, then, obtain, in addition to the factors and stages of production, the following grouping characteristics: parameters of the demand and supply functions and of their usual variations, duration of price and output reactions, degree of interchangeability. The measurement of these characteristics for each single representative commodity is in itself an econometric problem. A rough approach may, however, be suggested, even in the actual stage of research, by the following enumeration of rougher grouping or ranging principles: elasticity of demand as depending on prices, the responsiveness of demand to income changes (being one of the elements of the demand variability), duration of the price adaptations to monetary changes, duration of output adaptations, as known from technology. The "indices of instability" calculated by Mills for production of single commodities (average annual deviation from a constant growth rate calculated for a period) are complex characteristics reflecting the total effect of elasticity of demand and supply functions, their variability, and the duration of reaction. It will, however, probably not be impossible roughly to resolve these characteristics into their components—with knowledge of production and marketing techniques involved and with the aid of the elasticity figures obtained elsewhere.[2]

[2] Some of the highest and lowest "indices of instability" of production (Mills for 1901–1913), can be probably explained by the following dominating factors: (η = elasticity of demand, v_d = variability of demand, v_s = variability of supply, t = duration of reaction).

Low		*High*	
Wheat flour	1.7 (η, v_d, v_s)	Freight cars	33.6 (t, v_d)
Mfgd. tobacco and snuff	2.6 (v_d, v_s)	Passenger cars railroad	22.4 (t)
Lead	2.7 (?)	Apples	22.2 (v_s)
Natural gas	2.7 (η, v_d, v_s)	Raw tobacco	12.9 (v_s)
Fermented liquors	3.1 (v_d, v_s)	Motor vehicles	12.6 (η)
Raw wool	3.4 (v_s).		

III. MATERIAL

Three new publications, having a common aim of calculating national income, an American (King),[3] an English (Clark),[4] and a German (Statistisches Reichsamt),[5] may be selected as indicating the actual possibilities of statistically sketching Tableaux Économiques. A fourth publication, an American one (Mills),[6] while giving only a partial aspect of the movements of inter-group proportions, succeeds in developing interesting devices for their time analysis.

To a large extent these publications are based on previous ones[7] which they revise and subtantially enlarge. The value of such work greatly depends on the amount of material, and this is constantly being improved.[8] Other improvements depend on purely technical conditions of work; for instance, Clark, working single-handed, was handicapped in that he had to use smaller samples, etc., than King or the German Statistical Office; the latter had, however, no production census at their disposal.

It is repeatedly emphasized throughout each of the three books on national income that the figures contained therein are reliable for use in estimating the total, but must be employed with all possible caution

[3] *The National Income and Its Purchasing Power.* By Willford I. King, of the Staff of the National Bureau of Economic Research, Inc., assisted by Lillian Epstein. (Publications of the Nat. Bureau of Ec. Res. N2, 15, New York, 1930.)

[4] *The National Income 1924–1931.* By Colin Clark M.A., London, 1932.

[5] *Das deutsche Volkseinkommen nach dem Kriege.* Bearbeitet im Statistischen Reichsamt. (Einzelschriften zur Statistik des Deutschen Reichs Nr. 24), Berlin, 1932.

[6] *Economic Tendencies in the United States. Aspects of Pre-war and Post-war Changes.* By Frederick C. Mills. With an Introduction by the Committee on Recent Economic Changes. A Publication (Nr. 21) of the National Bureau of Economic Research in Cooperation with the Committee on Recent Economic Changes, New York, 1932.

[7] *U.S.A.: The Wealth and Income of the People of the U.S.*, by W. I. King (New York, 1923).—*Income in the U.S.*, by W. C. Mitchell, W. I. King, F. C. Macaulay, and O. W. Knauth (Publ. of the Nat. Bur. of Ec. Res.), 2 volumes.—*Income in the Various States*, by M. Leven and W. I. King (Publ. of the Nat. Bureau of Ec. Res.).—"The National Income and Its Distribution," by Morris A. Copeland (Chapter XII of *Recent Economic Changes in the U.S., Report of the Committee etc.*, Vol. II, New York, 1929).—For earlier works see the preface of the book on review.

England: A. L. Bowley and Sir Josiah Stamp. *The National Income 1924.* Oxford 1927.—A. W. Flux in *Journal of the Royal Economic Society*, 1929.

Germany: Helfferich. *Deutschlands Volkswohlstand 1888–1913.* Berlin 1917.—Rogowski, *Das deutsche Volkseinkommen.* Berlin 1926.—Current publications of the Institut für Konjunkturforschung and Statistisches Reichsamt.—*Comparisons:* Gini in Bull. de l'Inst. Int. de Statistique, 1931.

[8] Cf. Bowley's criticism on Clark (*Economica*, May, 1933), which defers the decision till three forthcoming official publications can be consulted.

as individual figures. However, these individual figures claim to be, and probably really are, the best available at the date of publication. It would, therefore, highly increase the value of the publications under review as sources of reference if these warnings were replaced by information as to the possible upper and lower limits of the estimates. Again, it is not only the partial estimates which cannot be safely used without such information; it would be equally dangerous, e.g., to substract the total national income from some other figure of a like order of magnitude when the possible limits are not known. It is to be regretted that the example of Bowley and Stamp has not been followed in this respect either in the English or in the German book. King had given some indications on error margins in a previous work, but dropped them in the recent one, and confined himself to some verbal characterizations (p. 96).

American statistics give probably the relatively best access to the different ways of computing the total income; they permit, therefore, also the greatest number of different grouping principles. Accordingly, King makes use of checking figures of net output, derived from production statistics, with figures on income-tax.[9] Yet another checking method, though theoretically highly desirable, is not yet performable even in the United States: The spending side is investigated by King (p. 374) only as far as commodities sold by retailers are concerned (production of final goods plus wholesalers' and retailers' margin) and is not checked by family budgets. Apart from some minor expenditure items, the savings present particular statistical difficulties, yet they are of particular theoretical interest. Budget statistics usually refer only to the poorer classes and consequently (savings being more responsive to income variations than most other budget items) cover but a very small part of national savings. Figures of capital market—issues, saving deposits, etc.,—as collected, e.g., for United States by Mills, Chapter IX, while on the one hand exaggerated by re-investings, such as share purchases by companies, have on the other hand no reference to the internal savings of enterprises.

Still more difficult is the approach from the side of commodities; although figures for the purchases of machinery and for the value of construction can be ascertained, the fraction needed for repair and replacement is largely arbitrary, and so is the valuation of inventories

[9] In a previous work (for the years 1909–18) the two ways were trusted to two different persons (O. W. Knauth and W. I. King). "The procedure used did not permit a comparison of results until both estimates were complete. It was an exciting moment when the sets of totals were brought together. All concerned were much gratified when it was discovered that the difference between the two estimates was in no year of much consequence" (p. 9).

in balance-sheets of representative companies. Both figures, the saving of money (including internal savings of enterprises) and the investment in goods, can, further, be said to check each other only if a complete business cycle period is taken; for single years, account should be taken of the difference between saving and investment. Clark's method of establishing this difference (Keynes's "profit") in an independent way will be dealt with separately; its result can, at any rate, hardly be checked by the direct substraction of savings from investment, owing to the gross margins of error contained in both these items. Yet I do not consider the situation as hopeless. The internal savings of companies have been on a wide basis analyzed by Mills (Chapter IX) and King (Chapter X); King's materials were probably the main basis of the estimates given by Copeland[10] for the business savings of non-company manufacture, commerce, and construction. The German publication under review gives new estimates of company savings for several years, based on fiscal revenue statistics (p. 53–4); if cautiously combined with capital statistics of companies and private enterprises of different sizes, as supplied by the Bank für Industrieobligationen,[11] these figures can substantially improve the numerous estimates of *Selbstfinanzierung* suggested in Germany in recent years. Clark's figures on undistributed profits (p. 138) are, however, not sufficiently supported. The difficult task of eliminating double reckonings in the capital market data, is attempted in a forthcoming research by W. Lederer. A careful scrutiny of all branches of production, made in Germany by the Institut für Konjunkturforschung,[12] arrived at a detailed presentation of new investments in fixed and circulating form, and of expenses on replacement. The results are at least not contradictory of permissible guess on total savings (and capital import) of Germany during a seven-year period, as derived from general saving statistics, and they agree with the data on total purchases of investment goods, i.e., supply of machinery, buildings, etc., and additions to stocks. The supply of investment goods, the due depreciation, and the movement of stocks, have also been subjects of Clark's research; the stocks item has been substantially improved by him since,[13] with the help of Bank of England statistics on retail trade stocks. A similar

[10] *Recent Economic Changes*, II, 781–810.

[11] *Zahlen aus Deutschlands Volkswirtschaft*, Berlin, 1932. (Figures based on capital assessments.)

[12] Sonderheft 22: "Kapitalbildung und Investitionem in der deutschen Volkswirtschaft" (by Keiser and Benning); its results have been revised and brought up to date in later publications of the Institute. *Cf. Konjunkturstatistisches Handbuch*, p. 48.

[13] *Economic Journal*, June 1933.

study for the United States would be worth trying and still easier, owing to smaller inter-census intervals, and the greater share covered by large enterprises with published accounts.

All these possible improvements in the statistics of new investment and saving combined with better information on retail trade, rent, and minor services brought by consumers would greatly help to check the other ways of constructing a *Tableau Économique*.[14] The statistics on "spending" thus being the least investigated, statistics on production and income give the main possible basis of research. King (p. 96) thinks, however, that not only agriculture, manufacturing, and mining, but also the incomes produced by transport, government activities, as well as the owners' share of the banking incomes, have been "fairly well authenticated" by him; his estimating methods for the remaining portions (covering, according to him, roughly one-half of the realized income) are unfortunately not being reported with sufficient precision. In England, similar attempts are probably still more difficult, so that Clark gives up the effort to control the "income method" by the "production method" and establishes the value of distributive and other services simply as a difference between the national income as shown by the income method, and the value of the finished agricultural and industrial product calculated on the lines of Flux. In the German research under review, the production method is only applied in the case of agriculture where the tax assessments prove to be totally unreliable. Valuable research towards a more complete picture of German net outputs is being made in current publications of the I. f. K.,[15] and a control of the income estimates from the production side is being prepared. The backbone of the total income statistics is still, at least in England and Germany, the Income Tax revenue and the wage inquiries.[16]

IV. DEFINING AND GROUPING

Much progress has been made, owing to better theoretical insight, towards an agreement on definitions. As Table I shows, differences in definition of the national income have practically either disappeared

[14] Cf. Nystrom. *Economics of Consumption: Economic Journal* 1931 (A. E. Feavearyear); *Die Arbeit* 1932 (W. Lederer); "Current retail turnover, Germany," in *Konjunkturstatistisches Handbuch*. For U.S.A. (indices only) see *Survey of Current Business*.

[15] Summarized in the *Konjunkturstatischen Handbuch*, p. 35 ff.; for distributive trades consider also the publications of the "Forschungsstelle für den Handel," Berlin, especially "Die Handelsspanne," bei Prof. Hirsch and Brandt.

[16] Tax exemptions and evasions, agriculture excepted, have been estimated by the Statistisches Reichsamt (for Germany, 1928) as about 3.5 per cent of the tional income. If we allow for this item an error of $\pm\frac{1}{3}$, it would mean ± 1.2 per cent national income.

TABLE I

SOME IMPORTANT BORDER CASES STATISTICALLY INCLUDED IN OR EXCLUDED FROM THE NATIONAL INCOME

Figures in brackets indicate percentages of the national incomes of the respective countries as defined in the German publication

	Statistisches Reichsamt (1928)	Clark (1924)	King (1925)	
			Realized income	Total income
1. Rental value of owned urban homes	Incl.	Incl.	Incl. (2.3)	Incl. (2.3)
2. Interest on investments in other durable consumption goods	Excl.[1]	Excl.[1]	Incl. (4.0)	Incl. (4.0)
3. Interest on those debts of the Central Government invested in buildings, etc.	(0.01) {Incl. / Excl. (post-war)	Excl. / Excl. (7.5)	Incl. / Incl. (0.8)	Incl. / Incl. (0.8)
4. Interest on other debts of the Central Government.	Excl. (prewar)			
5. Taxes expended for health, housing and education	Incl.	Incl.	Incl.	Incl.
6. Other taxes	Excl. (3.5)	Excl. (1.8)	Incl.	Incl.
7. Civil Servants' pensions (Central and Local)	Incl. (2.5)	Excl.	Incl. (1.1)	Incl.
8. Changes of inventory prices	Incl.	Incl. 1925 (−5.5) 1926 (−1.8) 1927 (−2.0)	Excl.	Incl.
9. Changes of land and stock-exchange prices	Excl.	Excl.	Excl.	Incl. (1925 +4.6) (1924 +3.8) (1923 −2.0)
10. Undistributed profits (partly included in 8 & 9.)	Incl. (2.5)[2]	Incl. (4.2)	Excl. (6.1)[3]	Incl.
11. Private household work, women	Excl.	Excl. (estim. 25%)[4]	Excl.	Excl.
12. Private household work, men	Excl.	Excl.	Excl.	Excl.
13. Supplementary jobs	Incl. (0.3)[5]	Excl.	Excl.	Excl.
14. Allowances in non-money form (other than board and room)	Excl.	Excl.	Excl.	Excl.

[1] Interest on installment consumptive credits, however, probably included.
[2] See text.
[3] Computed from Mills, p. 429, Copeland, 781–812, and King, p. 277 ff.
[4] Clark, pp. 4, 15.
[5] Employees only.

(as between Clark and the Statistisches Reichsamt) or can at least be reasonably accounted for.

Thus, reasonably reliable international comparisons are now possible, although, of course the individual items are more difficult to compare internationally than the figures of total national incomes. International comparisons of both kinds supply also, if carefully made, devices for mutual control between the figures. Some main ratios (such as labor's share) prove to be remarkably alike in different capitalistic countries. The sign and absolute amount of differences may be judged as reasonable or not, with help of some merely partial and non-statistical knowledge. This knowledge can similarly enable us to interpolate roughly the data for a country which, in any given respect, "lies between" two other countries. The same is, of course, true of inter-temporal comparisons. All such comparisons cannot, however perfect the formal agreement in definition, yield results not biased by the influence of "institutional," economically irrelevant changes such as the substitution of marketable services for home services. Irving Fisher's concept of Income and Capital (as "flow" and "stock" of *satisfaction* respectively), which is the only one that completely eliminates institutional border lines and replaces them by purely economic and mathematically formulable notions, could not be fully applied by the statisticians who have been working in this field. The margin of error attached to the measurement of certain "border-cases" proves to be larger than the margin of error involved in a comparison between two countries or two years with institutional differences not *too* great. Much like the Fisherian definition of the total Income and Capital, the theoretical division of income shares going to well-defined productive factors could also never be closely followed by statistics. Yet, for instance, changes of the employers' share due to the growth of salaried company administrators, are, for econometric purposes, to be separated from the profit changes due to technical progress or to inflation. King and Copeland in splitting up incomes of social groups according to economic categories[17] have probably squeezed out of the available material all it could give.

Apart from this "institutional" dimming of the economic border lines, partly inevitable in the material, the grouping principles adopted in publications such as those under review, are often not so immediately fit to answer theoretical questions as they doubtless could be if properly worked out. Some traditional grouping principles could—at least in all cases where the composing elements, such as the figures

[17] For example, farmers' incomes (1926/7) = interest (+1759), +labor remuneration (+3410), −deficit in actual profit (owing to fall of estate prices) (−1717) (p. 781).

on single commodities, are ready at hand—be abandoned, and rational principles substituted.

For example: Mills's main divisions of commodities are: (1) raw, manufactured; (2) farm products, all others; (3) foods, non-foods; (4) consumption goods, capital equipment; (5) non-durable, semi-durable, durable goods. Of these divisions, (5) is perfectly rational and economically relevant; the results (striking differences in the instability index) are accordingly highly interesting and probably susceptible of further econometric analysis. (4) is undoubtedly better than the usual separation into "consumers' goods" and "producers' goods," which has never been handled sequentially enough to consider retail trade, personal services, etc., as the only "consumers' goods productions," and to range the other stages (wholesale trade, manufacture, production of raw materials) according to their distance from consumption. Mills's division would have led to still more interesting results if any attempt had been made to separate new investment from mere replacement. Now, as to (3), is it not a merely physiological distinction? Why should (p. 561) wheat and cigarettes be grouped together as against cotton products and coal? Again, if the distinctive principle is the greater urgency of need (an economic category), it would not hold good in the case of cigarettes, etc. The differences of demand elasticities are much better reflected in Mills (2), contrasting between farms and non-farms; probably it would not be so in countries without their own raw cotton production. Apart from this, the division of farms and non-farms is more of a sociological and political, than of an economic interest; this particularly, since the high variability of supply, which indeed is a measurable characteristic of most vegetable products taken individually, tends to disappear if the farm production as a whole is being observed. As to (1), viz., the dichotomy, "raw materials—manufactured goods," its economic meaning is not clear. Is the distinction to be made according to the ratio between the "value added" and the "total value?" This ratio is in agriculture no less than 70 per cent,[18] i.e., substantially more (the double perhaps!) than in the production of such manufactured goods (in Mills's classification) as cotton oil, distilled spirits, or bricks. Or does the discriminating principle lie in the, different distances from consumption? Then apples and eggs, etc., should not be among the raw materials, and pig iron and bricks among the manufactured goods. Only the consumption act, not the "beginning" of production, which lies indeed in infinity, can be used as a starting-point for measurement of the Böhm-Bawerkian "stages of production."

[18] Computed from: Copeland, p. 781; *Deutsches Volkseinkommen*, p. 51; *Konjunkturstatistisches Handbuch*, p. 35.

The following figures show how perfectly Mills' "index of instability" works if applied to clear-cut economic categories (or degrees) and how its force is lost if the commodities are grouped otherwise. It seems, indeed, as if it could be used as a test of whether a group is formed so as to allow the study of an economic question or not:

Mills' Indices of Instability (1901–13)

(1) raw—manufactured	3.8–4.7
(2) farms—non-farms	3.4–6.1
(3) foods—non-foods	4.0–4.7
(4) consumption—capital equipment	2.8–8.8
(5) Non-durable—semi-durable—durable	3.0–3.4–8.1

The use Mills makes of these indices and the growth ratios—together with similar and modified indices computed for prices, and with figures on price dispersion—as instruments of business cycle analysis cannot be discussed here. It seems that the postulate of an equal growth rate for all commodities (including investment goods?) as indicative of and favorable to economic stability is not convincing.

V. INCOME SHARES, PROFITS

The movements of the inter-group exchange ratios have been studied as changes of the shares of nominal income or as changes of the purchasing power per unit. Mills's further method, viz., dividing the production index of a commodity by the purchasing power of the value added by its manufacture, results obviously (as he rightly notes, p. 176) in the ratio

$$\frac{\text{general price index}}{\text{single commodity price index}},$$

this, however, only if the physical proportion between material and factors has changed.

The idea that the shares of original factors are proportional to their partial productivities, perfect competition being supposed, led Cobb and Douglas[19] to plot a surface

$$P = f(L, C) \tag{1}$$

where P = annual physical product, L = amount of labor occupied, C = amount of capital employed (deflated Census figures). The partial productivities being $\lambda = dP/P : dL/L$ and $\gamma = dP/P : dC/C$ respectively, and the rewards per factor unit, l and c respectively, $l/c = dP/dL : dP/dC$; hence $lL/cC = \lambda/\gamma$. The income shares lL and cC are known from data on money incomes. The authors believe the simplest assumed equation

[19] *American Economic Review*, 1930.

$$P = \text{const. } L^{\lambda}C^{\gamma}$$

sufficiently fits the data. They are aware of the handicaps lying not only in the imperfect status of the data (arbitrariness of capital figures as based on capitalization rates) but also in the facts themselves, such as lack of free competition. It may be added that, in fact, not the product, but the profit, is being maximized when the amounts of factors to be employed are being decided. To substitute profit for product in the equation (1), would, however, again, imply a separation of two incomes institutionally bound together. This is just being attempted by Clark. He tries to calculate the profit as the difference between output value and "costs, including normal capital earnings." His method may be described as follows. A series of n years is to be found, which can be supposed to fulfil the condition

$$\sum Q_i = 0 \qquad (i = 1, 2, \cdots n), \tag{2}$$

Q_i being the annual profits in the year i. The annual physical outputs $O_i (i = 1, 2, \cdots n)$ can be established as multiples of the basis-year-output O_1 by multiplying the values of all the partial outputs of this year (net industrial output, railway services, etc.) by suitable production-indices, and dividing their sum by O_1. On the other hand, the annual increment e_i of cost E_i (not the costs themselves, of course), viz. $e_i = E_{i+1} - E_i$ can be estimated as the sum of wages' increase, dwelling rents' increase, and increase of "normal capital earnings," this latter being put at $7\frac{1}{2}$ per cent of the capital increment (i.e. new issues and undistributed profits) of the preceding year. We have, in each year, if π_i is the "price index of the production as a whole:"

$$Q_i = O_i\pi_i - E_i \qquad (i = 1, 2, \cdots, n) \tag{3}$$

If condition (2) is fulfilled, E_1 and all E_i and Q_i can be found (O_i, π_i and e_i for all the years being known) because then

$$\sum O_i\pi_i = \sum E_i = nE_1 + \sum_1^{n-1}(n-1)e_i \quad (i = 1, 2, \cdots, n)$$

and consequently

$$E_1 = \frac{\sum_1^n O_i\pi_i - \sum_1^{n-1}(n-1)e_i}{n},$$

$$Q_i = O_i\pi_i - \sum_1^{i-1} e - \frac{\sum_1^n O_i\pi_i - \sum_1^{n-1}(n-1)e_i}{n} \tag{4}$$[20]

[20] Clark's derivation of the increment of E/O, say $\Delta(E/O)$, is neither possible nor necessary; perhaps a misprint.

The condition (2) is supposed by Clark to be fulfilled in the series of years 1924–29 (omitting the strike year 1926), on the ground that in these years an even upward trend of employment was accompanied by a rapid increase of population. In this case—so would my interpretation of Clark run—the windfall earnings accruing to entrepreneurs owing to rising prices and/or falling costs are already accounted for in Clark's $7\frac{1}{2}$ per cent assigned to new capital sinking; this would correspond to the formal elimination, occasionally occurring in Keynes's *Treatise on Money*, of those entrepreneurial incomes arising in a steadily growing society, from the short-period profits proper. The growth of employable population, in a capitalistic society, can expand employment in no other way than *via* falling unit-wages and rising consumption prices, and the difference would go to the entrepreneur. A "steady" growth of population can be defined as one which causes the difference between wages and sales per unit of capital to be constant; or a *vice versa* definition could be chosen. Steadiness defined in one of these ways, would be, then, the only discriminating quality between the "normal earnings" (after deducting interest?) and the "profit," since neither the price movement nor the relation to the difference between saving and investment—saving itself depending on the definition of normal earnings—supplies a satisfactory test. Clark's "profits" may, therefore, be interpreted as containing an arbitrary constant (or containing a function of an arbitrary constant). The movements of his profit-figures would still be of great interest. The curve showing the "incentive" relation between profits and physical outputs, plotted by Clark recently (*Economic Journal*, June 1933, p. 213) remains important, even should it be shifted—provided that the margins of error involved were not too great. Unfortunately, the Q_i of Clark prove to be of an order of magnitude not very different from the order of magnitude of e_i; the errors contained in e_i and accumulated in the expression $\Sigma_1^{i-1} e_i$ can jeopardize the results. This will be the case, for instance, if we count normal earnings (p. 129) not only on issues and undistributed profits—a limitation for which Clark gives no reason—but consider also all other parts of new investment (as given p. 138, last line).

All Souls College, Oxford

PARTIAL TIME REGRESSIONS AS COMPARED WITH INDIVIDUAL TRENDS

By Ragnar Frisch and Frederick V. Waugh[1]

I. INTRODUCTION

There are in common use two methods of handling linear trend in correlation analysis of time series data, first, to base the analysis on *deviations* from trends fitted separately to each original series, and, second to base the analysis on the original series without trend elimination, but instead to introduce *time* itself as one of the variables in a multiple correlation analysis. The first method may be called the individual trend method and the latter the partial time regression method.

There are certain misconceptions about the relative value of the two methods and about the kinds of statistical results that are obtained by the two methods. The following simple example illustrates the situation. Suppose we are studying the relation of sugar consumption to sugar prices. We have data representing total annual consumption and price for several consecutive years. There may be a strong upward trend in consumption due to population increase or to some other factor that changes or shifts the demand curve. If we want to study only the relation of consumption to price, naturally we must eliminate the trend from the consumption before using the data to measure demand elasticity.

But, suppose that, during the period studied, there has been a persistent tendency in sugar prices to decline and that this has caused a long-time increase in consumption. This fact may be just what we want to express when we speak of the relation of sugar consumption to price. This long-time connection between consumption and price may be even more important for our problem than the short-time connection. If so, we must not, of course, eliminate trends from the variables before proceeding to the analysis. On the contrary, in this case the trend variation in price and the trend variation in consumption must be left in the material and *should be allowed to influence the regression coefficient* between consumption and price.

A common idea seems to be that this can be obtained by using the partial time regression method instead of the individual trend method. This is false. The possibility of determining the long time relation

[1] Mr. Waugh is a fellow of the Social Science Research Council and the present study was made in connection with research in Europe made possible by his fellowship.

considered in the above example does not depend on which of the two methods is used, but on a certain criterion regarding the variability type of the material at hand (the criterion is discussed below). The partial trend regression method can *never*, indeed, achieve anything which the individual trend method cannot, because the two methods lead by definition to *identically* the same results. They differ only in the technique of computation used in order to arrive at the results.

This illustrates one of several misconceptions that exist in this field, but there are also others; the various misunderstandings may be briefly classified into the following three points:

1. The significance of the regression coefficients as determined by the two methods. In particular, there exists a misconception as to the meaning of these coefficients as approximations to the underlying "true" relationship between the variables.

2. The significance of the correlation coefficients as determined by the two methods.

3. The significance of the trends. This last question is of particular interest from the point of view of forecasting.

Before proceeding to a more systematic discussion of these points we shall, by quotations from well known statisticians, illustrate the nature of the misunderstandings.

II. QUOTATIONS

The following are excerpts from an article by Bradford B. Smith "The Error of Eliminating Secular Trends and Seasonal Variation before Correlating Time Series," in the *Journal of the American Statistical Association*, December, 1925. He says:

> Should these two series, dependent and independent, chance to have approximately similar trend or seasonal movements, and should these latter be extracted from the two series prior to their correlation, one might under the name of seasonal and trend extract much of the variation by which the two were related, and thus obscure their true relationship. The unconsidered practice of eliminating trend and seasonal from series prior to their correlation is to be looked upon askance, therefore. It is often a serious error.
>
> The escape from this predicament involves no new theoretical consideration. . . . All that is necessary is to remember that fundamentally a numerical description of passage of time is merely taken to represent the magnitude of the combined effect of otherwise unmeasured factors and then this series of "magnitudes" is treated precisely as any other independent factor. (In Bradford Smith's paper "time" is treated as a factor in a multiple regression equation and partial coefficients of regression are obtained to indicate the relation of the dependent variable to this trend and to the other factors). . . . In following this practice, as often occurs, proper methods go hand in hand with better results. On theoretical considerations, correlation coefficients secured by *simultaneous*, or multiple, correlation methods will be as high or higher, and never less, than those resulting from any possible sequence of *consecutive* elimination of the influ-

ence of independent factors from the dependent, of which current methods of eliminating seasonal variations *before* correlating are an example. In actual trials of the two methods the writer has found that the simultaneous solution for trend and seasonal regression curves and curves for other factors always give markedly higher correlations. . . .

Mordecai Ezekiel in his book *Preisvoraussage bei landwirtschaftlichen Erzeugnissen* published in the series of the Frankfurter Gesellschaft für Konjunkturforschung, 1930, says (page 23):— "Oft wird is notwendig sein, den zusammengesetzten Einflusz dieser Kräfte, die mit dem Kalender variieren, zu messen; . . . allerdings ohne dass man dabei der stets vorhandenen Gefahr unterliegen darf, den Einflusz von zeitlich variienden Faktoren Veränderungen zuzuschreiben, die in Wirklichkeit auf andere Ursachen zurückzerfuhren sind. Aus dieser Grunde wird der Einflusz der zeitlich variierden Faktoren am besten durch eine Bestemmung des Trends in der Relation und nicht durch Feststellung desselben bein jeden eizelnen Faktor gewessen."

III. WHAT IS THE "TRUE" RELATIONSHIP

Proceeding now to a more exact statement of the problem, we must first consider the meaning of a "true" relationship, and in what sense such a "true" relationship can be approximated by various empirical methods.

When comparing the results of different methods in time series analysis one must keep clearly in mind the *object* of the analysis. It must be specified which sort of influence it is desired to eliminate, and which sort to preserve. Unless this is specified it has no meaning to say that a certain method will yield a "truer" relationship than another.

Such an expression has a meaning only if it is referred to a given theoretical framework. An empirically determined relation is "true" if it approximates fairly well a certain well-defined *theoretical* relationship, assumed to represent the nature of the phenomenon studied. There does not seem to be any other way of giving a meaning to the expression "a true relationship." For clearness of statement we must therefore first define the nature of the *a priori* relationship that is taken as the ideal.

Let us express this in mathematical terms. We consider a number of variables $x_0, x_1, \cdots, x_n$, the last of them, i.e., x_n, denoting time. We conceive *a priori* of a relation that expresses x_0 in terms of the other variables, if, for simplicity, the relation is assumed linear, it may be written

$$x_0 = \beta_{01\cdot 2\cdots n} \cdot x_1 + \cdots + \beta_{0n\cdot 12\cdots n-1} \cdot x_n \tag{3.1}$$

where the β's are constants and the variables are measured from their

respective means. Each of the constants in (3.1) is conceived so as to represent an *independent* influence on x_0, for instance, the constant $\beta_{0n\cdot 12\cdots n-1}$ represents the independent influence which time may exert directly on x_0, regardless of the particular way in which the other variables $x_1 \cdots x_{n-1}$ happen to evolve. A theoretical relation postulated *a priori* in this way we may call a *structural* relation. The coefficient $\beta_{0n\cdot 12\cdots n-1}$ in (3.1) we may call the partial, structural coefficient between x_0 and time.

Besides the structural relation (3.1) postulated by theory we shall now consider certain relations obtained by the classical statistical procedures. First we consider the regression of x_0 on the other variables, determined by the usual partial regression method, i.e. the relation

$$x_0 = b_{01\cdot 2\cdots n}\cdot x_1 + \cdots + b_{0n\cdot 12\cdots n-1}\cdot x_n \tag{3.2}$$

where the b's are the usual regression coefficients, $b_{0n\cdot 12\cdots n-1}$ we may call the partial trend coefficient of x_0.

On the other hand we may fit to each variable a straight line trend, i.e., a trend of the form

$$x_i = b_{in}x_n.$$

The coefficients b_{in} we call the individual trend coefficient of x_i $(i=0, 1 \cdots n)$. Further we consider the deviations

$$x'_i = x_i - b_{in}x_n$$

and determine the regression of x'_0 on $x_1' \cdots x'_{n-1}$.

This relation is of the form

$$x_0' = b'_{01\cdot 2\cdots n-1}\cdot x_1' + \cdots + b'_{0,n-1\cdot 1\cdots n-2}\cdot x'_{n-1}. \tag{3.3}$$

The coefficients b' in (3.3) we call the deviation-from-trend coefficients, or shorter the b' coefficients, and the coefficients b in (3.2) we call the time-as-a-variable coefficients, or shorter the b coefficients.

The first question we want to raise is: Will the b' coefficients be significant expressions for the structural coefficients, i.e., for the β coefficients? If not, will the b coefficients be more significant expressions for the β coefficients? On this point the misunderstanding seems to be particularly great. The belief seems to exist—the quotations given indicate this—that if trends are present in the variables $x_1 \cdots x_{n-1}$ the coefficient b' will not measure correctly the β, but the coefficient b will. This is entirely wrong. The correct answer is this: If the material at hand satisfies a certain variability condition, then either the b' or the b may be taken as approximations to the β, while if this variability condition is not fulfilled, neither the b' nor the b will repre-

sent the β. The b and the b' are always identical, the two methods only represent different ways of computing the same magnitudes.

The variability condition envisaged is this: Each variable x must contain (beside the linear trend and beside the irregularities that are not systematically connected with x_0) an independent non-linear component. In other words, the deviation of x_i from its linear trend must be *significant* from the point of view of the connection between x_i and x_0. And, furthermore, this fluctuation must not be linearly dependent on the deviations of the other variables from their linear trends.

This means that if the relation between x_o and the other variables is assumed linear, it is *by definition impossible to discriminate* between that part of a linear trend in x_o that is caused by linear trends in the other variables and that part which is caused independently by the flow of time. Either we have to leave both these parts of the linear x_o trend in the data, or we have to eliminate both parts. In the first case the regression coefficients between x_o and the other variables will be influenced amongst other factors by whatever independent linear shift there has been in x_0 over the period studied. In the second case this linear shift is eliminated but at the same time all long-time (linear) relations between x_0 and the other variables are eliminated, so that the regression coefficients between x_0 and the other variables will be determined only by the short time fluctuations.

These criteria are consequences of the considerations developed by one of the present authors in an earlier publication.[2] We shall not give any formal proof of these criteria here. We shall confine ourselves to showing that the coefficients b and b' are identical by definition. This will of course be sufficient to exhibit the fallacy of the belief that anything more can be accomplished by the partial trend regression method than by the method of individual trends.

IV. THE IDENTITY OF THE REGRESSIONS DETERMINED BY THE INDIVIDUAL TREND METHOD AND THE PARTIAL TIME REGRESSION METHOD

We consider first the case of two variables (with time as the third variable). Let there be observed a set of values $x^{(1)}, x^{(2)} \cdots$ and $y^{(1)}, y^{(2)} \cdots$ of the two variables x and y at the points of time $t=t^{(1)}, t^{(2)} \cdots$, and let these values be expressed as deviations from their respective means. That is to say, we have $\sum x=0$, $\sum y=0$, $\sum t=0$, where the summations are extended to all the observations.

The moments of the variables we denote

[2] Ragnar Frisch: "Correlation and Scatter in Statistical Variables," *Nordic Statistical Journal*, August 1929, pp. 36—103.

$$
\begin{aligned}
(4.1) \qquad & m_{xx} = \Sigma x^2 \qquad m_{yy} = \Sigma y^2 \qquad m_{tt} = \Sigma t^2 \\
& m_{xy} = \Sigma xy \qquad m_{xt} = \Sigma xt \qquad m_{yt} = \Sigma yt.
\end{aligned}
$$

Let us first measure the relation of y to x by the individual trend method. If linear trends are fitted separately to x and y by the ordinary least squares procedure, the regression coefficient of x on t is m_{xt}/m_{tt} and the regression coefficient of y on t is m_{yt}/m_{tt}. If the deviations from trend are denoted x' and y' we consequently have

$$
(4.2) \qquad x' = x - \frac{m_{xt}}{m_{tt}} \cdot t
$$

$$
(4.3) \qquad y' = y - \frac{m_{yt}}{m_{tt}} \cdot t
$$

and the regression coefficient of y' on x' is

$$
(4.4) \qquad b_{y'x'} = \frac{\Sigma \left(x - \frac{m_{xt}}{m_{tt}} \cdot t \right) \cdot \left(y - \frac{m_{yt}}{m_{tt}} \cdot t \right)}{\Sigma \left(x - \frac{m_{xt}}{m_{tt}} \cdot t \right)^2}
$$

Expanding this we get

$$
(4.5) \qquad b_{y'x'} = \frac{m_{tt}m_{xy} - m_{xt}m_{yt}}{m_{tt}m_{xx} - m_{xt}^2}.
$$

Now let the data be analysed by the partial time regression method with y as the dependent variable and x and t as the independent variables. The regression coefficients of y on x and t are then found by the solution of the following simultaneous equations:

$$
\begin{aligned}
(4.6) \qquad & m_{xx}b_{yx\cdot t} + m_{xt}b_{yt\cdot x} = m_{xy} \\
& m_{xt}b_{yx\cdot t} + m_{tt}b_{yt\cdot x} = m_{ty}.
\end{aligned}
$$

Multiplying the second equation by m_{xt}/m_{tt} and subtracting from the first we get

$$
(4.7) \qquad m_{xx} - \frac{m_{xt}^2}{m_{tt}} b_{yx\cdot t} = m_{xy} - \frac{m_{xt}m_{yt}}{m_{tt}}
$$

that is

$$
(4.8) \qquad b_{yx\cdot t} = \frac{m_{tt}m_{xy} - m_{xt}m_{yt}}{m_{tt}m_{xx} - m_{xt}^2}.
$$

Similarly we obtain

$$
(4.9) \qquad b_{yt\cdot x} = \frac{m_{xx}m_{yt} - m_{xy}m_{xt}}{m_{tt}m_{xx} - m_{xt}^2}.
$$

But (4.8) is identically the same as (4.5).

Furthermore, (4.9) is identically the same as the coefficient one would get in *estimating* y by the individual trend method. Indeed, if at the point of time t, the independent variable had the value x, then the estimated value y of the dependent variable would be determined as follows. First one would determine the trend value of y as $m_{yt}/m_{tt} \cdot t$, and to this one would add (4.5) times the estimate of deviation from trend in x, which is equal to $(x - m_{xt}/m_{tt} \cdot t)$. The total estimate of y would consequently be

$$(4.10) \qquad y = \left(x - \frac{m_{xt}}{m_{tt}} \cdot t \right) \cdot \frac{m_{tt}m_{xy} - m_{xt}m_{yt}}{m_{tt}m_{xx} - m_{xt}^2} + \frac{m_{yt}}{m_{tt}} \cdot t$$

which reduces to

$$(4.11) \qquad y = \frac{m_{tt}m_{xy} - m_{xt}m_{yt}}{m_{tt}m_{xx} - m_{xt}^2} \cdot x + \frac{m_{xx}m_{yt} - m_{xy}m_{xt}}{m_{tt}m_{xx} - m_{xt}^2} \cdot t.$$

The coefficient of x in this expression is identical with (4.8) and the coefficient of t is identical with (4.9). Therefore *the regression equations given by the two methods are identical.*

A Numerical Example

These results can easily be checked by setting up a simple problem and solving it by the two methods. The following computations may serve as an example.

Observed data

x	t	y
−5	−3	+6
−3	−2	+5
−2	−1	+1
+2	0	+2
+1	+1	−3
+4	+2	−5
+3	+3	−6

Moments about the mean

$m_{xx} = 68$ $\quad m_{xt} = 41$ $\quad m_{xy} = -84$

$m_{tt} = 28$ $\quad m_{ty} = -60$

Trend coefficients

$$b_{xt} = \frac{41}{28} = 1.464$$

$$b_{yt} = \frac{-60}{28} = 2.143$$

By the individual trend method we consequently get:

Trend values		*Deviations*	
Of x	Of y	x'	y'
−4.393	+6.429	− .607	− .429
−2.929	+4.286	− .071	+ .714
−1.464	+2.143	− .536	−1.143
0.000	0.000	+2.000	+2.000
+1.464	−2.143	− .464	− .857
+2.929	−4.286	+1.071	− .714
+4.393	−6.429	−1.393	+ .429

$$b_{y'z'} = \frac{m_{x'y'}}{m_{x'z'}} = \frac{3.858}{7.964} = 0.484.$$

And by the method of partial time regression we get:

$$\begin{aligned} 68\,b_{yx.t} + 41\,b_{yt.x} &= -84 \\ 41\,b_{yx.t} + 28\,b_{yt.x} &= -60 \\ \hline 60.036\,b_{yx.t} + 41\,b_{yt.x} &= -87.857 \\ 7.964\,b_{yx.t} &= 3.857 \\ b_{yx.t} &= 0.484 \quad \text{(The same value as } b_{y'z'}\text{)} \end{aligned}$$

Consider now the n variables $x_0 \cdots x_{n-1}$ and let time be an $(n+1)$th variable x_n. Let all the variables be measured from their means so that $\sum x_i = 0$ $(i = 0, 1 \cdots n)$ where $\sum$ denotes a summation over all the observations. Let $m_{ij} = \sum x_i x_j$ be the moment of the variable x_i with x_j. The regression of the variable x_k on all the others is the linear equation obtained by replacing the row $m_{k0}, m_{k1} \cdots m_{kn}$ in the moment determinant by $x_0, x_1 \cdots, x_n$ and equating the result to 0. In particular, if x_0 is selected as the dependent variable, the regression will be

$$\begin{vmatrix} x_0 & x_1 & \cdots & x_n \\ m_{01} & m_{11} & \cdots & m_{1n} \\ \cdots & \cdots & \cdots & \cdots \\ m_{n0} & m_{n1} & \cdots & m_{nn} \end{vmatrix} = 0. \tag{4.12}$$

On the other hand, if we fit to x_i $(i = 0, 1 \cdots n-1)$ by least squares a straight line in x_n, we obtain the equation $x_i = m_{in}/m_{nn} \cdot x_n$, so that the deviations from trend will be

$$x_i' = x_i - \frac{m_{in}}{m_{nn}} \cdot x_n. \tag{4.13}$$

The regression of x_0' on $x'_1 \cdots x'_{n-1}$ is consequently

$$(4.14) \qquad \begin{vmatrix} x'_0x'_1 & \cdots & x'_{n-1} \\ m'_{10}m'_{11} & \cdots & m'_{1,n-1} \\ \cdots & \cdots & \cdots \\ m'_{n-1,0}m'_{n-1,1} & \cdots & m'_{n-1,n-1} \end{vmatrix} = 0$$

where $m'_{ij}=\sum x_i'x_j'$ are the moments of the deviations. We proposed first to show that the regression coefficient of x_0 on x_i $(i=1, 2 \cdots n-1)$ in (4.12) is identically the same as the coefficient of x'_0 on x'_i in (4.14).

By virtue of (4.13) the moments of the deviations are expressed in terms of the original moments thus:

$$(4.15) \qquad m'_{ij} = m_{ij} - \frac{m_{in}m_{nj}}{m_{nn}} \qquad (i, j = 0, 1 \cdots n).$$

Obviously $m'_{ij}=0$ whenever $i=n$ or $j=n$, or both $i=n$ and $j=n$.

We subtract from the row m_{i0}, $m_{i1} \cdots m_{in}$ $(i=1, 2 \cdots n-1)$ in (4.12) m_{in}/m_{nn} times the last row. The resulting expression will be the determinant obtained from (4.12) by replacing all the moments m by m' except those in the last row. In particular, the moments in the last column thus obtained will be zero, except the last element of the last column which is maintained equal to m_{nn}.

In other words (4.12) takes the form

$$(4.16) \qquad \begin{vmatrix} x_0 & x_1 & \cdots & x_{n-1} & x_n \\ m'_{10} & m'_{11} & \cdots & m'_{1,n-1} & 0 \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ m'_{n-1,0} & m_{n-1,1} & \cdots & m'_{n-1,n-1} & 0 \\ m_{n0} & m_{n1} & \cdots & m_{n,n-1} & m_{nn} \end{vmatrix} = 0.$$

But the determinant (4.14) may be enlarged to

$$(4.17) \qquad \begin{vmatrix} x_0' & x_1' & \cdots & x'_{n-1} & 0 \\ m'_{10} & m'_{11} & \cdots & m'_{1,n-1} & 0 \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ m'_{n-1,0} & m'_{n-1,1} & \cdots & m'_{n-1,n-1} & 0 \\ c_0 & c_1 & \cdots & c_{n-1} & 1 \end{vmatrix} = 0$$

where $c_0, c_1 \cdots c_{n-1}$ is a set of arbitrary numbers, for instance m_{n0}, $m_{n1} \cdots m_{n,n-1}$. This shows that when we write (4.12) and (4.14) in the forms

$$(4.18) \qquad a_0x_0 + a_1x_1 + \cdots + a_nx_n = 0$$

and

$$a_0'x_0' + a_1'x_1' + \cdots + a'_{n-1}x'_{n-1} = 0 \tag{4.19}$$

then the first n coefficients $a_0 \cdots a_{n-1}$ in (4.18) will be proportional to the coefficients $a_0' \cdots a'_{n-1}$ in (4.19). Indeed, the a-coefficients will simply be equal to m_{nn} times the a'-coefficients. We consequently have

$$b_{0i.12\cdots n} = b'_{0i.12\cdots n-1} \qquad (i = 1.2 \cdots n-1) \tag{4.20}$$

where b and b' are the coefficients of (3.2) and (3.3) respectively.

Furthermore it is clear that we can take the regression equation (3.3) as a starting point, insert here the values $x_i' = x_i - b_{in}x_n$ and thus get a regression expressing x_0 in terms of $x_1 \cdots x_n$. This latter regression is identical with the regression (3.2). This follows from (4.20) and from the well known fact that

$$b_{0n} = b_{01.2\cdots n}b_{1n} + b_{02.13\cdots n}b_{2n} + \cdots + b_{0n.12\cdots n-1}b_{nn} (b_{nn} = 1). \tag{4.21}$$

Thus the complete regression equation determined by the partial time regression method and the method of individual trends are identical, (3.2) and (3.3) are only two ways of writing the same equation. This also has a bearing on the meaning of trends as a means of forecasting, this aspect of the problem we shall take up in Section 6.

The way in which we pass from (4.12) to (4.16) is in reality nothing but an application of the Gaussian algorithm for solving the normal equations.[3]

This throws an interesting light on the connection between (4.14) and (4.16) and also on the nature of the Gaussian algorithm itself. It shows that the Gaussian algorithm simply consists in this: First one fits to all the variables, except x_n, a "trend" which is linear in x_n. Then to the deviations from trend in all the remaining variables one fits a new "trend" which is linear in the deviation x'_{n-1}, and so on. In this way one finally gets down to a two-variable problem. And this successive fitting of *individual* trends is identical with the determination of a simultaneous partial regression. *The identity of these two processess is just the basis of the Gaussian algorithm.*

V. THE CORRELATION COEFFICIENTS OBTAINED BY THE TWO METHODS

Another misunderstanding seems to be that the higher correlation coefficients obtained by the partial time regression method than by the individual trend method is an expression of the superiority of the first of

[3] Most of the "new" schemes for solving the normal equations that are developed from time to time are nothing but unessential modifications of the Gaussian algorithm. This applies, for instance, to the Doolittle method.

these two methods. The fact that a comparison of correlation coefficients in this case can lead to a belief in the "superiority" of a method that is by definition identical with the "inferior" method, is a striking example of the perfectly imaginary character of much of the reasoning that is currently based on correlation coefficients.

A coefficient of multiple correlation showing the total relationship between one variable such as prices and two others such as consumption and time, must *by its very definition* always be as high as or higher than the gross correlation between price and consumption expressed as deviations from their respective trends, in other words we always have by definition

$$r^2_{y.xt} \geqq r^2_{x'y'}. \tag{5.1}$$

Indeed it is easy to verify that

$$r^2_{y.xt} = 1 - \frac{\Delta_{xyt}}{\Delta_{xt}} \tag{5.2}$$

$$r^2_{x'y'} = 1 - \frac{\Delta_{xyt}}{\Delta_{xt}\Delta_{yt}} \tag{5.3}$$

where Δ_{xt}, Δ_{yt} and Δ_{xyt} denote the correlation determinants in the sets xt, yt and xyt respectively. These are positive definite determinants lying between 0 and 1. Any of them is equal to 1 when, and only when, all the variables in the set are uncorrelated.[4] The formulae (5.2) and (5.3) show that $r^2_{y.xt}$ is always larger than $r^2_{x'y'}$ *no matter what the data are* (provided only that they consist of real numbers). The only exception is the limiting case $r^2_{y.xt} = r^2_{x'y'}$ which occurs when, and only when, y and t are uncorrelated (disregarding the case where there exists a mathematically exact linear relationship in the set xyt). But that is not all. There is a *great* discrepancy between $r^2_{y.xt}$ and $r^2_{x'y'}$ when and only when nearly all the variation in y can be represented simply by a linear trend. From (5.2) and (5.3) we get indeed

$$\frac{1 - r^2_{y.xt}}{1 - r^2_{x'y'}} = \Delta_{yt} = 1 - r^2_{yt}. \tag{5.4}$$

In other words the only thing that a comparison between $r^2_{y.xt}$ and $r^2_{x'y'}$ can tell us *is whether y has a pronounced trend or not.* The comparison does not tell anything about the nature of this trend: whether it is due to a variation in x or to some other cause. It has no meaning as an indication of a "better result," it does not tell us anything about whether the b coefficients or the b' coefficients are the better approxima-

[4] See for instance "Correlation and Scatter."

tions to the "true" β coefficients (as implied for instance in the quotation from Bradford Smith).

Since the comparison of $r_{y \cdot xt}$ and $r_{x'y'}$ indicates only whether y has a pronounced trend or not, and since this fact will be revealed immediately from the plot of y, the computation of these correlations in the present problem will in our opinion serve no significant purpose. It is only a play with formulae which at best is superfluous, but at worst is dangerous, because it may lead to wholly unwarranted conclusions, such as, for example, the conclusion about the "superiority" of the time regression method.

Some concrete examples may illustrate this. In such studies as those concerned with consumption as a function of prices, price as a function of supply, or acreage as a function of previous price, *it is always possible to obtain high coefficients of multiple correlation by including in the simultaneous analysis such factors as time, population,* etc., whenever the period studied is one of considerable change in one of these factors.

One of the authors of the present article once published[5] the results of certain studies of the relation of vegetable prices to quality, based on several hundred observations which indicated total correlations of about 0.70 between certain qualities and the deviation of prices from the daily quotation on the market. He was criticized in a review by Dr. Bean who claimed that the correlation coefficients were too low to be significant. The critic remarked that he had found from experience that correlations of at least 0.90 were necessary for significant results in price analysis: and well he might, because he was working with annual data of supplies and prices covering from eight to ten observations and using multiple curvilinear correlation methods, including in the analysis such factors as trend. In the case of the study of quality, it would have been an easy matter to raise the coefficient of multiple correlation high enough to suit such a critic, indeed it could have been raised practically as near to unity as one pleased by the simple expedient of including as an independent factor in the analysis the daily quotation for standard quality. This was avoided in order to get a truer statement of the actual relation between quality and price.

We do not wish to object to the various results obtained by Dr. Bean, but we do want to point out the uselessness of comparing results of time series analyses merely on the basis of multiple correlation coefficients.

VI. THE MEANING OF THE TRENDS

In the structural relation (3.1) each variable exerts an *independent* influence on x_0. Hence, if trends are present in $x_1 \cdots x_{n-1}$, this will

[5] F. V. Waugh: *Quality as a Determinant of Vegetable Prices.* New York, 1929.

contribute towards the total trend in x_0. This total trend in x_0 may be looked upon as made up of the indirect trends produced by the trends in $x_1 \cdots x_{n-1}$ and of the partial trend caused by the last term in (3.1). Let us express this mathematically remembering the fundamental distinction between the structural coefficients β and the empirically determined regression coefficients b. If x_i is expressed in the form

$$x_i = x_i' + \beta_{in} x_n \qquad (x_n = \text{time}) \qquad (i = 1, 2 \cdots n)$$

where β_{in} is a constant and x_i' the deviation from trend in x_i, then (3.1) may be written in the form

$$x_0 = \beta_{01.2\cdots n} x_1' + \cdots + \beta_{0,n-1.12\cdots n} x'_{n-1} + \beta_{0n} x_n \tag{6.1}$$

where the coefficient β_{0n} is equal to

$$\beta_{0n} = \beta_{01.2\cdots n}\beta_{1n} + \beta_{02.13\cdots n}\beta_{2n} + \cdots + \beta_{0,n-1.12\cdots n}\beta_{n-1,n} + \beta_{0n.12\cdots n-1}. \tag{6.2}$$

β_{0n} is the *total* structural trend coefficient for x_0, β_{in} are the *individual* structural trend coefficients for the variables $x_i \cdots x_{n-1}$; the products $\beta_{0i.12\cdots n}\cdot\beta_{in}$ are the *indirect* trend coefficients in x_0; and $\beta_{0n.12\cdots n-1}$ the *partial* (and direct) trend coefficient in x_0.

If, in the material at hand, the deviations $x'_1 \cdots x'_{n-1}$ from (linear) trends exhibit systematic, linearly, independent variations, the structural coefficients β in (3.1) may be determined by the data, the empirically determined regression coefficients b may then be taken as approximations to the structural coefficients β with the same subscripts.

Consequently if the coefficients $b_{0i.12\cdots n}$, and the individual coefficients b_{in} $(i = 1 \cdot 2 \cdots n)$ $(b_{nn} = 1)$, are determined empirically we have (on the assumption that the x'-s deviate significantly from linear trends) representations for all the terms in the right member of (6.2), we may consequently say that we also have determined empirically an approximation to the *total* trend coefficient for x_0. Let b_{0*} be the total trend coefficient thus determined by inserting b for β in the right member of (6.2). In other words, b_{0*} is defined as the composite effect of the trends in $x_1 \cdots x_{n-1}$ and the partial trend of x_0, these latter trends being determined by the usual regression procedure.

We may of course also consider the *individual* trend coefficient for x_0 just as for the other variables. If this individual coefficient is determined by the usual regression procedure, i.e. as the coefficient b_{0n} *it becomes just equal to* b_{0*}. This simply follows from (4.21). And from (4.20) it follows that we get exactly the same determination of b_{0*} whether in the right member of (6.2) we use b_{in}' as approximations to β_{in} or we use b_{in}.

We may express this by saying that the partial trend coefficient for x_0 may be determined either by the partial time regression method directly as the usual coefficient $b_{0n\cdot 12\ldots n-1}$ or it may be determined as the difference between total trend in x_0 and the trends in x_0 that are ascribed to the influence of the various independent factors, *these latter being determined by the individual trend method.* Also, with respect to the interpretation of the trends, the two methods yield consequently exactly the same results, the only difference being a difference in the technique of computation. The fact here discussed is of course only another aspect of the fact proved in Section 4, namely that the complete regression equations determined by the two methods are identical.

The above conclusion may be illustrated by the case of two variables x and y with time t as a third variable. The partial trend coefficient in y is $b_{yt\cdot x}$, and the trend effect in y computed by the individual trend method is equal to $b_{yt}-b_{xt}\cdot b_{y'x'}$, i.e., total trend in y minus indirect trend in y attributed to the fact that y depends on x, this latter dependence being determined by the individual trend method. But according to the general formula (4.21) we just have $b_{yt\cdot x}=b_{yt}-b_{xt}b_{y'x'}$. This checks in the numerical example in Section 4 since $-2.143-(1.464)\cdot(0.484)=-2.852$.

How do these considerations affect the use of trends in forecasting? There are three possible procedures for extrapolating a trend into the future: (1) we can project the total trend observed in x_0, simply using the angular coefficient b_{0n}; (2) we can project the partial trend observed in x_0, namely the trend defined by the coefficient $b_{0n\cdot 12\ldots n-1}$; or (3) we can make what we may call a *composition forecast* of the trend in x_0 by forecasting each of the variables $x_1\cdots x_{n-1}$ and from these forecasts and the relation between x_0 and $x_1\cdots x_{n-1}$ and the partial trend in x_0, build up a total forecast of the trend in x_0.

If we are to make such a composition forecast of x_0 we must make individual forecasts of $x_1\cdots x_{n-1}$ and also of the partial trend in x_0. If each of these forecasts is made by the usual regression methods on the basis of the information contained in the material at hand, *the composition forecast of the trend in x_0 will be exactly the same as the direct mechanical projection of the total trend of x_0.* This follows immediately from the above considerations (see in particular formulae (4.21)). In other words the whole process of determining partial regression coefficients in this case is an entirely unnecessary roundabout calculation. This is another aspect of the identity between partial time regression and the individual trend method.

The only reason for using a composition forecast instead of a mechanical projection of the total observed trend in x_0 is to make it pos-

sible to utilize some *other information* regarding the probable future trends of the variable $x_1 \cdot \cdot \cdot x_{n-1}$. Instead of assuming that all the variables, $x_1 \cdot \cdot \cdot x_{n-1}$, will continue their growth at the average rate observed in the material, we may, for instance, assume that one of them is going to remain stationary at the level it had at the end of the period studied, or we may assume that its growth rate will become less, etc. If such specific guesses are made about the variables $x_1 \cdot \cdot \cdot x_{n-1}$, the composition forecast for x_0 using the regression (3.2) will not, of course, be identical with the direct mechanical forecast.

To illustrate the difference between a direct mechanical forecast of the total trend and a composition forecast, let x_0 be the consumption of sugar. If we fit a trend line to sugar consumption over the interval of time for which we have observations, and simply project this line into the future, we are assuming that the trends in population, sugar prices, and other factors influencing sugar consumption, will continue at the same rate that we have observed in the past. In some cases such an assumption is admissable, and in others it is not. For example, prices may have dropped so low during the period studied that further declines are very improbable, so that in forecasting the trend of sugar consumption we must reckon with a probable change in the trend of sugar prices which will, in turn, influence the total trend of consumption. To forecast the trend of consumption in this case, we need to know more than its average rate of increase in the past; we need also to know how much of this increase was due to the decrease in prices, so as to be able to say what the trend would have been had prices remained at a level corresponding to that expected in the future. In other words we must use a composition forecast with more or less plausible guesses regarding the future trends of the variables which influence sugar consumption.

Or, to take another example, if x_0 is the yield of potatoes, and $x_1 \cdot \cdot \cdot x_{n-1}$ represent weather data such as rainfall and temperature, in many cases we have no reason for expecting a real trend in the weather data, and probably can make no better forecast than to assume that the weather factors will vary around averages based on past observations. In this case the trend in x_0 which we would project would be the partial trend $b_{0n.12 \ldots n-1}$, which as we have shown is the same as the total trend in x_0 minus the trend in x_0 attributed to the factors $x_1 \cdot \cdot \cdot x_{n-1}$. The projection of this partial trend may, then, be considered as one form of the composition forecast in which we forecast that the various independent factors will not continue their observed rate of growth but will vary around their observed average.

Statistical Seminar, University of Norway

THE EDGEWORTH TAXATION PHENOMENON

By Raymond Garver

In a number of papers[1] Edgeworth, the eminent English economist, has given illustrations of an interesting and paradoxical taxation phenomenon. The situation is as follows. A monopolist is offering two commodities, one of which is a cheaper substitute for the other, in the same market. Thus, the only creamery in a town may offer two grades of butter, or the owner of the only theater may have seats at two different prices. A governmental tax of so much per unit of commodity is now imposed on the more expensive of the two commodities; what will be the effect on the selling prices set by the monopolist, assuming always that he is interested in making his profit as large as possible?

Edgeworth's examples are designed to show that it may be (though admittedly it would ordinarily not be) to the advantage of the monopolist to lower both his selling prices. That is, the monopolist not only passes none of the burden of taxation on to the consumer, but actually assumes an additional burden. Edgeworth's phenomenon has a fairly important place in mathematical economics; Hotelling[2] has written, "Edgeworth's paradoxical discovery . . . might very well be brought out in a course in mathematical economics. This is one proposition of economic importance which everyone can appreciate but which cannot be proved, apparently, without the use of formulae; moreover, it is very suggestive of further developments."

In the present paper I wish to question the validity of Edgeworth's illustrations, by developing certain implications of the demand functions which he employs. The method is necessarily mathematical, though the final criticism itself is of an economic nature. I then wish to present another example which seems to me to illustrate the desired phenomenon in a more satisfactory manner.

I shall consider first what seems to be the last of Edgeworth's examples,[3] since it is simpler than the others. The symbols x and y represent the amounts of two commodities, sold by the same monopolist, which will be bought in a unit of time when the prices are p_1 and p_2 respectively, p_1 being greater than p_2. Edgeworth gives the prices as functions of x and y, though it seems a little more natural, in the case of a monopolist, to think of the prices as the independent variables and the amounts demanded or purchased as the functions. His relations are

[1] *Papers relating to Political Economy*, I, 132, 148; II, 93, 124, 400.
[2] *American Math. Monthly*, XXXVIII (1931), 103.
[3] *Op. cit.*, II, 401.

$$(1)\qquad \begin{aligned} p_1 &= £1\cdot\left[\frac{17}{5}\left(\frac{x}{100,000}\right) - 2\left(\frac{x}{100,000}\right)^2 - \frac{2}{5}\left(\frac{y}{200,000}\right)\right], \\ p_2 &= £\frac{1}{2}\cdot\left[\frac{73}{30} - \frac{22}{15}\left(\frac{y}{200,000}\right) + \frac{13}{30}\left(\frac{y}{200,000}\right)^2 - \frac{2}{5}\left(\frac{x}{100,000}\right)\right]. \end{aligned}$$

For simplicity, the assumption is made that the cost to the monopolist of supplying these commodities is a constant C, so that his profit is given by $V = xp_1 + yp_2 - C$. The usual methods of the calculus suffice to show that this profit will be a maximum when $x = 100,000$, $y = 200,000$, $p_1 = £1$, $p_2 = £1/2$. It is also possible to show that, if a small tax is imposed on the more expensive commodity, it will be to the advantage of the monopolist to lower both of his prices. The reader will find the details of these calculations given in the original source.

Edgeworth devotes some space to the justification of equations (1); some justification is of course essential, since the relations are by no means self-explanatory. He gives the properties which he considers fundamental for such a pair of demand functions, and shows that equations (1) possess these properties, at least over a wide enough range of values to suit his purposes. I believe, however, that a deeper investigation will bring to light an undesirable feature.

Let us differentiate equations (1) partially, first with respect to p_1, then with respect to p_2, regarding x and y as functions of p_1 and p_2. Four equations are thus obtained, and if we substitute in them the optimum values of x and y, that is, 100,000 and 200,000 respectively, they become

$$(2)\qquad \begin{aligned} 3\frac{\partial x}{\partial p_1} + \frac{\partial y}{\partial p_1} &= -500,000, \\ 4\frac{\partial x}{\partial p_1} + 3\frac{\partial y}{\partial p_1} &= 0, \\ 3\frac{\partial x}{\partial p_2} + \frac{\partial y}{\partial p_2} &= 0, \\ 4\frac{\partial x}{\partial p_2} + 3\frac{\partial y}{\partial p_2} &= -2,000,000. \end{aligned}$$

These equations are satisfied by $\partial x/\partial p_1 = -300,000$, $\partial y/\partial p_1 = 400,000$, $\partial x/\partial p_2 = 400,000$, $\partial y/\partial p_2 = -1,200,000$. The first of these means that, at least at $x = 100,000$, x is a decreasing function of p_1; the second means that y is an increasing function of p_1, and so on. Such simple properties as these are among those considered desirable by Edgeworth.

However, we note the additional fact that $\partial x/\partial p_1 + \partial x/\partial p_2$ is positive, and this, it seems to me, is not desirable. This is its economic interpretation: if the monopolist raises p_1 and p_2 (from the values £1 and £1/2 respectively) each by the same (sufficiently small) amount, he will be able to sell *more* of the more expensive commodity than before. For the change in x caused by small changes in p_1 and p_2, say Δp_1 and Δp_2, is given approximately by $(\partial x/\partial p_1)\Delta p_1 + (\partial x/\partial p_2)\Delta p_2$. Such a result, however, is scarcely plausible. People who buy the cheaper article do so because the saving more than counterbalances the loss in quality; a rise in the price of the cheaper article alone will lessen the price differential, and will cause a decrease in the amount of the cheaper article sold, and an increase (though usually not an equivalent increase) in the amount of the more expensive article sold. If both prices rise by the same amount, the price differential is not changed; the normal result is for sales of both articles to decrease. There is no real incentive for users of the cheaper article to change to the more expensive, even though it is true that the *ratio* of the lower price to the higher has increased.

A second example which Edgeworth also develops in detail[4] is one where the relations between price and demand are slightly more complicated, namely

$$
\begin{aligned}
p_1 &= £5 \cdot \left[\frac{602}{375} - \frac{1}{5}\left(\frac{x}{20{,}000}\right) - \frac{2}{3}\left(\frac{x - 19{,}200}{20{,}000}\right)^{3/2}\right. \\
&\qquad \left. - \frac{1}{2}\left(\frac{y}{100{,}000}\right)\right], \qquad (3) \\
p_2 &= £1 \cdot \left[\frac{431}{110} - 2\left(\frac{y - 69{,}750}{100{,}000}\right)^{1/2} - \frac{1}{2}\left(\frac{x}{20{,}000}\right)\right].
\end{aligned}
$$

If the monopolist's cost of production is again constant, the maximum profit obtains when $x = 20{,}000$, $y = 100{,}000$, $p_1 = £4.5$, $p_2 = £2.318+$. And again it can be shown that the imposition of a small tax on the more expensive commodity will cause the monopolist to lower both prices. The criticism here is the same as before. It requires but a minute's calculation to show that, corresponding to the optimum values of x, y, p_1, p_2, we have $\partial x/\partial p_1 = -320{,}000/21$ while $\partial x/\partial p_2 = 440{,}000/21$. Their sum is again positive.

Edgeworth's third example[5] is a modification of his second. He uses equations (3), but with the factor 5 on the right hand side of the first

[4] *Ibid.*, I, 148.
[5] *Ibid.*, I, 132.

equation omitted, with $x/20{,}000$ replaced by x, and with $y/100{,}000$ replaced by y. These simplifications actually give quite a different result; the maximum profit is now obtained when $x=1$, $y=1$, $p_1=£.9$, $p_2=£2.318+$. The cheaper commodity is taxed this time, though Edgeworth's discussion on page 134 does not mention this fact. But the demand functions are subject to the same criticism as before; when $x=y=1$, $\partial y/\partial p_1=22/21$, $\partial y/\partial p_2=-88/105$, and their sum is positive.

In the attempt to obtain Edgeworth's phenomenon and yet avoid this criticism, I was naturally led to the use of simpler demand functions, whose implications are fairly obvious. It then turned out that some attention had to be paid to cost of production; Edgeworth had tried to put the whole burden of his work on his demand functions.

I assume that the amounts of the two commodities which will be purchased in a unit of time when the prices are p_1 and p_2 are given by

$$\begin{aligned} x &= cp_1 + d + kp_2, \\ y &= ap_2 + b + lp_1. \end{aligned} \tag{4}$$

For these equations to be suitable from the economic standpoint it is clear that d, b, k, l, should be positive constants, while c and a must be negative constants. I think it will also be agreed that $c+k$ should be negative, and that $a+k$ should be negative. For, as has been mentioned above, if p_1 and p_2 are increased by the same amount, we should expect less of the more expensive commodity to be sold (per unit of time) than before. (Under certain conditions, however, it seems plausible that more of the cheaper commodity might then be sold.) And, if p_2, the price of the cheaper commodity, be increased without changing p_1, we should expect the total amount sold, that is, $x+y$, to diminish. For, while y will decrease and x increase, not all of the decreased demand for the cheaper commodity will be translated into increased demand for the more expensive. (But a change in p_1 alone *might* increase the total amount sold, since the customers attracted to the cheaper commodity by the increased price difference might easily use more units of it than they had of the more expensive.)

I assume that the cost of producing x units of the more expensive commodity and y units of the cheaper, per unit of time, is given by $Ay^2+By+C+Dx$, where B, C, D are positive, and A may be positive or negative.[6] If it is negative, however, the cost function will be valid only when y is less than $-B/2A$, that is, when Ay^2+By is an increasing function of y. The monopolist's profit per unit time is then given by $\pi=xp_1-yp_2-\text{cost}$, so that π is a simple quadratic function of p_1 and p_2

[6] The term Ay^2 seems to be essential to our purposes, while a term in x^2 is not.

which the reader can write down explicitly. The profit will be maximized when p_1 and p_2 make

$$\frac{\partial \pi}{\partial p_1} = \frac{\partial \pi}{\partial p_2} = 0,$$

provided

$$\frac{\partial^2 \pi}{\partial p_1^2} < 0, \quad \frac{\partial^2 \pi}{\partial p_2^2} < 0, \quad \left(\frac{\partial^2 \pi}{\partial p_1^2}\right)\left(\frac{\partial^2 \pi}{\partial p_2^2}\right) > \left(\frac{\partial^2 \pi}{\partial p_1 \partial p_2}\right)^2.$$

These last three conditions reduce to

$$\begin{gathered} 2c - 2Al^2 < 0 \\ 2a - 2Aa^2 < 0 \\ 4ac(1 - Aa) + 4Aalk - (l + k)^2 > 0. \end{gathered} \tag{5}$$

We shall denote the left hand side of this last equation by Δ.

Now the equations $\partial \pi/\partial p_1 = \partial \pi/\partial p_2 = 0$ can be solved at once, giving

$$\begin{aligned} p_1 &= D[(2ac - k^2 - kl) - 2Aa(ac - kl)] + \text{other terms}/\Delta, \\ p_2 &= D[c(k - l) + 2Al(ac + kl)] + \text{other terms}/\Delta. \end{aligned} \tag{6}$$

These prices maximize the profit, provided conditions (5) are satisfied. We assume that our constants are chosen so that the p_1 and p_2 determined by (6) are positive.

The imposition of a tax of so much per unit on the more expensive commodity, to be paid by the monopolist, is simply equivalent to an increase in D, in the cost function. For the optimum prices to both decrease when D is increased the two coefficients of D in (6) must be negative. These conditions, as well as those imposed after (4), those of (5), and the rather indefinite condition that our optimum prices are positive, are all satisfied by taking $c = -12$, $a = -10$, $k = 9$, $l = 6$, $A = -1/12$, $B = D = 10$, C arbitrary, say zero, $d = 114$, $b = 75$.

The reader may now consider this special case in detail, without reference to the general conditions which we have derived. He will find that the maximum profit is obtained when p_1 is fixed at 18 and p_2 at 15. Thirty-three units of each commodity will be sold per unit of time. If a tax of 1/2 per unit of the more expensive commodity be now imposed, raising D to 10.5, the monopolist will find it advantageous to lower p_1 to 17.93 and p_2 to 13.54. To the nearest unit, x is now 21 and y is 47. The Edgeworth phenomenon is thus obtained.

Perhaps a word may be said in justification of the cost function $-y^2/12 + 10y + C + 10x$. The overhead, of course, is C. Then each unit of x costs 10 to produce. The cheaper commodity, on the other hand,

is subject to the law of decreasing cost; when y units are produced, the cost of each is $10-y/12$. This is certainly not unreasonable. We may mention again that the cost function will not be valid for $y>60$, but this range is ample for our work.

The reader may also find it interesting to set $A=0$. He will find that it then becomes impossible to satisfy all the conditions which have been imposed.

University of California at Los Angeles

NOTE ON EDGEWORTH'S TAXATION PHENOMENON AND PROFESSOR GARVER'S ADDITIONAL CONDITION ON DEMAND FUNCTIONS

By HAROLD HOTELLING

IN A recent paper[1] I derived for demand functions for n commodities conditions which, I concluded, assure the satisfaction of all further conditions which may reasonably be applied to *all* sets of demand functions. Letting $p_1, p_2, \cdots, p_n$, and $q_1, q_2, \cdots, q_n$ be respectively the prices and quantities, the conditions are:

$$\frac{\partial p_i}{\partial q_j} = \frac{\partial p_j}{\partial q_i}, \quad \frac{\partial p_i}{\partial q_i} < 0, \qquad \frac{\partial(p_i, p_j)}{\partial(q_i, q_j)} > 0, \qquad \frac{\partial(p_i, p_j, p_k)}{\partial(q_i, q_j, q_k)} < 0, \cdots.$$

These are equivalent to

$$\frac{\partial q_i}{\partial p_j} = \frac{\partial q_j}{\partial p_i}, \quad \frac{\partial q_i}{\partial p_i} < 0, \qquad \frac{\partial(q_i, q_j)}{\partial(p_i, p_j)} > 0, \qquad \frac{\partial(q_i, q_j, q_k)}{\partial(p_i, p_j, p_k)} < 0, \cdots,$$

and were known, at least in part, by Edgeworth. Similar conditions in which the inequality signs are all $>$ apply for supply functions.

One important feature of these conditions is that they are invariants. Thus if we change the units of measure, or even replace the commodities by bundles containing mixtures of them in varying proportions, these conditions, if satisfied originally, will be satisfied by the new units or the new bundles. It seems reasonable that any set of conditions of universal application, as distinguished from the characteristics of particular commodities, should be invariants, just as in physics it is accepted that a law of nature should not depend on particular coordinates but should be capable of invariantive expression. Certainly it is obvious that general economic principles should at least be independent of the question whether one commodity is measured in miles and another in ounces.

Professor Garver's new condition, which we may write in the same notation,

$$\frac{\partial q_1}{\partial p_1} + \frac{\partial q_1}{\partial p_2} < 0,$$

is not an invariant, even under changes in the unit of measure of one of the commodities. It is therefore not of universal application to pairs of commodities, though individual cases can be found in which it

[1] "Edgeworth's Taxation Paradox and the Nature of Demand and Supply Functions," *Journal of Political Economy*, XL (1932), p. 590.

seems reasonable. But the seeming reasonableness may arise in an illusory way from the accidental use of units having the same name for measuring different commodities. The fact that butter and margarine are both sold by the pound does not mean that a pound of margarine is a substitute for exactly a pound of butter. For consumers who will pay as much for two pounds of margarine as for one pound of butter, equal small increases in price will increase the consumption of butter, in violation of Professor Garver's condition. A somewhat similar point is dealt with on pp. 595–596 of the paper cited above.

Since the new condition is applicable only in particular cases, its non-satisfaction in a hypothetical example does not show the example unrealistic. The examples which Edgeworth gave, and the additional examples in my paper cited above, cannot be attacked on this ground.

The necessity of a cost function to produce Edgeworth's phenomenon does not follow from the new condition, for this is satisfied by the example of my equations (40). It arises rather from arbitrarily taking the demand functions to be linear.

In spite of these qualifying considerations, Professor Garver's paper is of real value in pointing out a condition which may well be expected in numerous cases. He has also performed a service for those concerned with the statistical evaluation of demand functions in emphasizing that theoretical conditions exist with which empirical results may be compared.

Columbia University

EFFECTS OF A TECHNOLOGICAL IMPROVEMENT ON EMPLOYMENT

By D. I. Vinogradoff

In factories, in production, machines replace men—such is the basis of our progress. Electricity, water, coal, wind, and other forces of nature, perform tasks which in olden times were performed by the nerves and muscles of human beings. More time and human energy become available for spiritual life, the standard of living is continuously rising. Due to the fact that social progress does not coincide with technological progress, that there is a time-lag between a technological step forward and the necessary readjustment, this process is not painless. If, at a certain moment, men, actually replaced by machines, continue their productive work and enjoy life, there is no pain connected with their replacement; if they do not, they suffer, and the whole of society is affected, to some degree, by their suffering.

In a capitalistic society, any technological improvement requires change, and the purpose of the following is to analyze the effects of technological improvements under various conditions. Since industrial phenomena are extremely complicated, certain assumptions and simplifications are unavoidable. The results will be as applicable to life as any pure theoretical reasoning to everyday facts—they at least help us to separate various elements and to determine their interdependence.

THE INDIVIDUAL INDUSTRY

In every industrial enterprise there are three main functions—making the product, distributing (selling) it, and giving service to customers using it. In addition to these, there are the purchasing of raw materials, financing and accounting, protecting the enterprise legally, taking care of employees, etc. Let us designate, per day—

	Before technical improvement	*After technical improvement*
demand	D	D_1
total output	U	U_1
output per workman on machine	u	u_1
unit cost	c	c_1
unit price	p	p_1
number of workmen at machines	n	n_1
number of workmen assisting	n_0	n_{01}
number of white-collar employees in the factory	w	w_1
number of employees in distribution	d	d_1
number of employees in service	s	s_1
total number of employees	e	e_1

Obviously, $e=n+n_0+w+d+s$.

We assume that $p \dot{-} \dot{-} c$ and that $U = D$, i.e., that there is no waste and no time-lag. The number of workmen at machines making the product is the next higher integer to the fraction U/u; the possible difference is so slight that we can take $n = U/u = D/u$. The number of workmen serving and assisting in the shops, i.e., storing materials, moving them to the machines, moving the product, storing it, serving more complicated machines, is proportional to the total output U; $n_0 \dot{-} \dot{-} U$; let a be the coefficient of proportionality. Therefore $n_0 = aU = aD$. The number of "white collar" employees in the factory, i.e., those doing clerical work connected with the product, engineering, central accounting, purchasing, advertising, etc., is also proportional to the demand; let us write $w = bD$. The number of employees connected with the distribution, i.e., moving the output to distribution offices, storing, selling, doing local accounting, etc., is also proportional to the demand $d = fD$. The same is true for the service force: $s = gD$, so that the total number of employees

$$e = \frac{D}{u} + aD + bD + fD + gD = \frac{D}{u} + kD.$$

We can see that the total number of employees may be considered as consisting of two parts, in one of which n depends upon the output per workman u, and in the other is independent of u; let us designate this part by n_s;

$$n_s = n_0 + w + d + s; \qquad e = n + n_s.$$

Suppose, now, that due to a technological improvement, the output per workman rises from u to u_1; the unit cost c of the product will be c_1, smaller than c; we can sell it for p_1 instead of p, $p_1 < p$. The normal reaction of the market will be an increased demand, $D_1 > D$; then we have to produce more, $U_1 > U$, and to change our force. The problem is to find e_1 as a function of e and other quantities. We assume all these changes to be instantaneous. The new set of quantities will be as follows:

$$n_1 = \frac{D_1}{u_1} = n\frac{D_1 u}{D u_1}$$

$$n_{s1} = kD_1 = n_s\frac{D_1}{D}.$$

$$e_1 = n\frac{D_1 u}{D u_1} + n_s\frac{D_1}{D} = \frac{D_1}{D}\left(n\frac{u}{u_1} + n_s\right).$$

Let $n_s/n=t$, or $n_s=tn$. Then

$$e = n(1+t), \quad e_1 = n\frac{D_1}{D}\left(\frac{u}{u_1}+t\right).$$

The number of employees in our industry will be unchanged ($e_1=e$) when $n(1+t)=n\,D_1/D(u/u_1+t)$, or[1]

$$\frac{D_1}{D} = \frac{1+t}{\dfrac{u}{u_1}+t}.$$

Suppose $t=1$.

$$\frac{D_1}{D} = \frac{2}{\dfrac{u}{u_1}+1}.$$

Example.—Suppose the output increases 5 times, $u_1=5u$; then $D_1=1.67\,D$. If the demand increases 67 per cent, the number of employees remains the same; if $D_1>1.67\,D$, more employees will be hired, if $D_1<1.67\,D$, fewer employees are necessary. Two limit cases should be considered, namely, (Case I) when the market is very far from saturation, demand is unlimited, the distribution force is small, and the number of workmen on machines exceeds all others so much that we may consider $n_s=u$. Then

$$e_1 = e\frac{D_1 u}{D u_1}.$$

[1] This relation can be presented in the curve form as follows:

$$\frac{D_1}{D} = \frac{1+t}{\dfrac{u}{u_1}+t} = \frac{u_1+u_1 t}{u+u_1 t},$$

$D_1u+D_1u_1t=Du_1+Du_1t.$
Let $D_1=D+\Delta D$, $u_1=u+\Delta u$.
$(D+\Delta D)\,u+(D+\Delta D)\,(u+\Delta u)u=D(u+\Delta u)+D(u+\Delta u)\,t.$
$Du+\Delta Du+Dut+\Delta Dut+D\Delta ut+\Delta D\Delta ut=$
$Du+D\Delta u+Dut+D\Delta ut$
$\Delta Du+\Delta Dut=D\Delta u,$
$\Delta D+\Delta Dt/D=\Delta u/u,\ \Delta D/D(1+t)=\Delta u/u.$
$(1+t)dD/D=du/u.$
$(1+t)1gD=lg\,u,\ D(1+t)=u+\text{Const.}$
If $t=1$, then $D^2=u$, i.e., the curve representing this relation is parabola. If $t=0$, the relation is linear.

The number of employees can remain unchanged only when the demand is inversely proportional to the output. This is the case usually considered in textbooks and in articles dealing with the subject of technological unemployment in a conversational way. To emphasize the effect of price, usually the elasticity of demand is introduced into the discussion. Elasticity of the demand E is the coefficient of proportionality showing the relation between price and demand.[2]

$$\frac{D_1}{D} = E\frac{p}{p_1}, \qquad E = \frac{D_1 p_1}{Dp}.$$

The demand is neither elastic nor inelastic if it changes inversely to the price; then $E = 1$. Suppose we do not introduce any improvement in machinery, $u_1 = u$, but at a certain stable E, we lower the price; the result will be a greater demanded quantity $D_1 = E\ D\ p/p_1$, and we will need more employees.

$$e_1 = e\frac{D_1 u}{D u_1} = e\frac{D_1}{D} = eE\frac{p}{p_1}.$$

(Case 2) When u_1 is so much greater than u that practically $u : u_1$ equals zero; then $n_1 =$ zero and

$$e_1 = nt\frac{D_1}{D}; \qquad e_1 = e \text{ when } \frac{D_1}{D} = \frac{1+t}{t} = 1 + \frac{1}{t}.$$

If in the beginning $t = 1$, i.e., $n = n_s$, and suddenly an improvement changes u into u_1 so that $u : u_1 =$ zero, and $n =$ zero, then the number of employees will be unchanged if the demand increases in accordance with the formula:

$$\frac{D_1}{D} = 1 + \frac{1}{t} = 2, \text{ i.e., if the demand doubles.}$$

All employees will work on jobs covered by the designation n_s—assisting, clerical, engineering, purchasing, distribution (sales), and service. If the demand does not double, part of the employees will be laid off (or will work for smaller wages). If the demand does not increase at all, only half of the former number of employees are necessary. All the above considerations relate to one specific industry.

EFFECT OF EXPENDITURE REDUCTION

Let us now analyze the effect of a technological improvement on the market outside this specific industry. When we lower the price, our

[2] The author deliberately uses a simplified expression for E, different from that determined for the case of continuous change of demand.

old customers reduce expenses. The amount reduced equals $(p-p_1)D = (p-p_1)U$, and this is an additional purchasing power free on the market. If the market is inelastic, this additional purchasing power will be counter-balanced by the loss of purchasing power of the persons laid off in our industry; but if the elasticity of the market equals unity or more, and there are no persons laid off, this additional purchasing power will improve the market still more. Under normal conditions only a very small amount of this expenditure reduction is wasted or hoarded; most of it is usefully spent on consumers' goods or again invested, directly or through the banks, in industrial enterprises. In order to have an idea of the effect of such investment, let us suppose that the whole of the above saving is usefully invested in an enterprise similar to ours, i.e., with the same values of u, p, U, etc., or, for simplicity's sake, in the same enterprise. We wish to determine how many employees can be occupied by the money saved. We can proceed in the following manner.

The quantity P of the product which can be purchased for this saving $p=(p-p_1)U/p_1$; to handle this product n' workmen at machines and n_s' employees on distribution, service, and other functions, are necessary.

$$n' = \frac{P}{u_1} = \frac{(p-p_1)U}{p_1 u_1} = \frac{(p-p_1)un}{p_1 u_1} = n\frac{u}{u_1}\left(\frac{p}{p_1}-1\right).$$

The quantity each employee handles equals

$$\frac{D_1}{n_{s1}} = \frac{D_1}{n_s\dfrac{D_1}{D}} = \frac{D}{n_s} = \frac{u}{n_s}.$$

Therefore

$$n_s' = \frac{\dfrac{P}{U}}{n_s} = n_s\frac{P}{U} = n_s\frac{(p-p_1)}{p_1} = n_s\left(\frac{p}{p_1}-1\right).$$

Price p_1 covers both functions (as well as profits). The total number of employees on the market in addition to our enterprise is

$$e' = n' + n_s' = n\frac{u}{u_1}\left(\frac{p}{p_1}-1\right) + n_s\left(\frac{p}{p_1}-1\right)$$

$$= \left(\frac{p}{p_1}-1\right)\left(n\frac{u}{u_1}+n_s\right)$$

Combining both figures, those in the industry under consideration and the others on the outside market, we will have

$$e_{tot} = e_1 + e' = n\frac{D_1 u}{D u_1} + n_s\frac{D_1}{D} + \left(\frac{p}{p_1} - 1\right)$$

$$= \left(\frac{p}{p_1} - 1\right)\left(\frac{n^u}{p_1} + n_s\right)$$

$$= n\frac{D_1 u}{D u_1} + n_s\frac{D_1}{D} + \left(\frac{D_1}{ED} - 1\right)\left(n\frac{u}{u_1} + n_s\right)$$

$$= \frac{D_1}{D}\left(n\frac{u}{u_1} + n_s\right) + \left(\frac{D}{ED} - 1\right)\left(n\frac{u}{u_1} + n_s\right)$$

$$= \left(n\frac{u}{u_1} + n_s\right)\left(\frac{D_1}{D} + \frac{D_1}{ED} - 1\right)$$

$$e_{tot} = e \text{ when } n + n_s = \left(n\frac{u}{u_1} + n_s\right)\left(\frac{D_1}{D} + \frac{D_1}{ED} - 1\right)$$

$$\text{or } (1 + t) = \left(\frac{u}{u_1} + t\right)\left(\frac{D_1}{D} + \frac{D_1}{ED} = 1\right)$$

$$\frac{D_1}{D}\left(1 + \frac{1}{E}\right) = \frac{1 + t}{\dfrac{u}{u_1} + t} + 1.$$

To obtain a clearer view of these relations let us take an example. Suppose $t = 1$ and $E = 1$. The above formula will read:

$$\frac{D_1}{D}(1 + 1) = \frac{1 + 1}{\dfrac{u}{u_1} + 1} + 1$$

$$\frac{2D_1}{D} = \frac{2}{\dfrac{u}{u_1} + 1} + 1, \quad \frac{D_1}{D} = \frac{1}{\dfrac{u}{u_1} + 1} + \tfrac{1}{2}.^3$$

Suppose

$$\frac{u}{u_1} = u.1; \quad \frac{D_1}{D} = \frac{1}{1.1} + 0.5 = 1.49.$$

[3] In Curve form this can be presented as $D^1 = u + \text{Const.}$

That is, if the output per machine increases 10 times, under conditions assumed ($E=1$ and $u=1$) the demand need increase only 49 per cent in order that the total number of employed remain unchanged. Let us consider two limit cases, viz., n_s+zero, and u/u_1=zero.

(Case 1)

$$e_{tot} = n\frac{u}{u_1}\left(\frac{D_1}{D}+\frac{D_1}{ED}-1\right) = n\frac{u}{u_1}\left[\frac{D_1}{D}\left(+1\frac{1}{E}\right)-1\right]$$

e_{tot} remains unchanged, when

$$\frac{u}{u_1}\left[\frac{D_1}{D}\left(1+\frac{1}{E}\right)-1\right]=1,$$

$$\frac{D_1}{D}\left(1+\frac{1}{E}\right)=\frac{u_1}{u}+1,$$

$$\frac{D_1}{D}=\frac{1+\dfrac{u_1}{u}}{1+\dfrac{1}{E}};\quad \text{Suppose } E=1,$$

$$\frac{D_1}{D}=\tfrac{1}{2}\left(\frac{u_1}{u}+1\right)$$

(Case 2)

$$e_{tot} = n\left(\frac{D_1}{D}+\frac{D_1}{ED}-1\right)$$

$$e = n + n_s = n_s\left(1+\frac{1}{t}\right)$$

$$e_{tot} = e, \text{ when } \frac{D_1}{D}+\frac{D_1}{ED}-1 = 1+\frac{1}{t},$$

$$\frac{D_1}{D}=\frac{1+\dfrac{1}{t}+1}{1+\dfrac{1}{E}}=\frac{2+\dfrac{1}{t}}{1+\dfrac{1}{E}};\ \text{if } E=1,$$

$$\frac{D_1}{D}=\frac{2+\dfrac{1}{t}}{2}=1+\frac{1}{2t}.$$

If $t=1$, then $\dfrac{D_1}{D}=1.5.$

This does not mean that an unlimited amount of product will be distributed at $D_1 = 1.5\ D$ but that if n diminishes to zero, the same number of employees will be used on the market when the demand increases to 1.5 of its former value.

Various industries in our country have reached different stages of mechanization; some of them, particularly smaller local enterprises, are still near Case 1; others are near Case 2—almost fully mechanized. The trend is toward a complete mechanization: industry as a whole needs less and less hand-work and more and more men for distribution and service. Machines become more automatic, and the work on them more brain work than hand work; moreover, contemporary requirements for precision of work necessitate complete cleanliness of shop and of workman; shops acquire the aspect of laboratories, and soon the phrase "white-collar job" will lose its meaning. This is a profound change in industrial life, and a corresponding readjustment in our social structure should follow with the least possible delay. A considerable time-lag between these two phenomena may have very grave, even disastrous, results. Discussion of this point is outside the scope of the present article.

The above mathematical analysis considers an instantaneous effect of a technological step forward. Industrial progress consists in a series of such "jumps," and, if considered for a number of decades, may present a continuous process. Mathematical investigation of it would require application of differential calculus; it would be necessary to introduce infinitesimal changes of u, D, and E, i.e., du, dD, and de; demand as a function of price is then represented as a curve $Dp^E = \text{Constant}$, where E is the elasticity of demand; there would be curves representing $c = f(u)$, $p = f(c)$, etc. This is a problem different from the subject of this article.

In closing, the following conclusion should be emphasized: change in the total number of persons employed on the market, caused by a technological improvement, may be positive, zero, or negative, depending upon the demand. In times of rapidly increasing demand, the change is positive, and the above formulae determine this change; in times of dull demand, the change is negative. The change is always positive in case the technological improvement is so important that it opens up an entirely new industry; it has then the effect of an invention. Improvements in making automobiles practically started the automobile industry in the United States; the same is true in regard to the telephone industry.

Westinghouse Electric and Manufacturing Company

PSEUDO-SCIENTIFIC METHOD IN ECONOMICS

By Joseph Mayer

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1. A Seeming Impasse in Value Theory

Not long ago it was stated with some emphasis that a seeming impasse in value theory has been reached, that the systems and points of view of five leading contemporary Anglo-American economists, considered representative and outstanding in England and the United States, are rather bewildering in their contradictions and lack of concordance, and that no way out is in sight.[1]

The way out, if there is one, is assuredly the path pursued by other sciences in shaking off the incubus of pseudo-scientific method in the course of their development: by the geometers of ancient Greece who, following the analyses of Aristotle and Plato, freed themselves from Pythagorean mysticism in establishing the system of Euclid; by William Gilbert, who much later proved by painstaking experiment that neither goat's blood nor garlic had anything to do with the phenomena of magnetism; by Francis Bacon, who pled with his contemporaries to cease spinning "laborious webs of learning" out of "no great quantity of matter," and test their ideas against the actual facts of experience; by Robert Boyle, who urged students of chemistry to write plainly and unostentatiously about their experiments and thereby "keep men from being stunned, as it were, or imposed upon by dark and empty words." If an impasse has been reached in value theory, it may not seem wholly unwarranted to venture the suggestion that this phase of economic study may be in about the same position today that mathematics was in the days of Pythagoras, that physics was in the days of Gilbert, or that chemistry was in the days of Bacon and Boyle. These sciences have passed their periods of impasse and are making steady headway because they have learned to distinguish the scientific from the pseudo-scientific, to regard hypotheses as tentative and of no existential worth unless or until verified, and to eschew dogma of whatever variety.

Such reflections, it should be added, if pertinent at all, are meant to apply mainly to value theory with its subjective implications, and not

[1] Paul T. Homan, *Contemporary Economic Thought* (Harper's, 1928), pp. 439–468; also, "Issues in Economic Theory," *Quarterly Journal of Economics*, May, 1928, pp. 333–365.

to so-called pure theory with its mathematical analysis of objectively considered phenomena, as, e.g., of supply and demand, price movements, taxes and tariffs; interest, capital, and income; purchasing power and money. The most important current advances in economic method would seem to consist, on the one hand, in just such mathematical applications—in pure theory, curve fitting, correlation, probability, and the like—so admirably summarized by Irving Fisher in his address before the American Association for the Advancement of Science three years ago,[2] and, on the other hand, in clearing away some of the pseudo-scientific underbrush in value theory, which, therefore, needs to be sharply differentiated from (rather than integrated with) so-called pure theory.

It is fortunately becoming less necessary to deny the validity of the old contention that the methods of social study cannot fairly be compared with those of physical science, because of the greater difficulty of measuring social theories against social facts. The organization of such bodies as the Econometric Society with its purpose to apply to economic theory "constructive and rigorous thinking similar to that which has come to dominate in the natural sciences," is a sufficient answer to this contention.

Mathematical and inductive advances are doubtless fundamental and will sooner or later of their own momentum blast away any theoretical impasse.[3] At the same time, the day of liberation may be considerably hastened by a renewed examination of the various assumptions underlying generally accepted value theories, in the light of rigorous logic and modern socio-psychological knowledge, to the end that what is fallacious therein may be definitely discarded.

After some twenty years spent in such an examination, the speaker is inclined to the conviction that much of what still goes for recognized cost and utility theory is honeycombed with pseudo-scientific reasoning and sophistry. No economist can help theorizing with respect to value, whether he does it unconsciously or deliberately, so that the danger is

[2] Irving Fisher, "The Application of Mathematics to the Social Sciences," *Bulletin of the American Mathematical Society*, April, 1930, pp. 225–243; Cf. also articles by Arthur F. Bentley, entitled "Sociology and Mathematics," beginning in the July, 1931, issue of *The Sociological Review*, London, pp. 85 ff.; cf. also W. C. Mitchell, "Quantitative Analysis in Economic Theory," *American Economic Review*, XV (1925), pp. 1–12.

[3] Cf. G. C. Evans, *Mathematical Introduction to Economics*, New York, 1930, and recent articles, mentioned in his Appendix I, by Bowley, Edgeworth, Evans, Fisher, Hotelling, Moore, Roos, Schultz, *et. al.;* W. C. Mitchell, *Business Cycles, the Problem and its Setting*, New York, 1927, and other volumes (especially *The Behavior of Prices* by Mills), published by the National Bureau of Economic Research, Inc.; Irving Fisher, *The Making of Index Numbers*, Boston, 1922.

ever present of unwittingly using and passing on to younger generations of economists subtle fallacies which have long since in one form or another been shown in their true colors. The economic literature of the past forty years[4] contains many acute refutations of false analogies, hasty generalizations, and unwarranted assumptions, which nevertheless insist on reincarnating themselves. One reason why it seems so difficult to keep these pseudo-scientific ghosts laid, in addition to the support they often receive from positions held by great thinkers of the past, is that the refutations have as a rule been made in piecemeal fashion, and often lend color to one questionable dogma while excoriating another.

There have been giants of thought among the economists, from Adam Smith and before his time onward, and present-day followers are justly proud of past accomplishments. But knowledge of scientific fundamentals was much more meagre 100 to 150 years ago than it is in our day, and to point out in what respect old hypotheses were crude or inadequate in the light of present understanding should not be regarded as a disparagement. Other disciplines have passed through a process of rigorous scrutiny of handed-down conceptions in moving towards scientific maturity, and there seems to be no legitimate reason why economics or any other social study should be regarded as an exception. There is still too much dependence upon the authority of past utterances in economics, whereas no such attitude maintains today in the physical or biological sciences. Theories once seriously held by Pythagoras, Aristotle, Euclid, Ptolemy, Galen, Linnaeus, Newton, and other outstanding thinkers in recognized scientific fields, have been utterly discarded, or greatly modified, without disturbing the places their proponents occupy in history. In fact, science would seem to have advanced in proportion as it has learned to profit by the mistakes of its most important benefactors. It was only as this lesson was generally appreciated after the Middle Ages that the scientific renaissance became possible.

Homan's picture of contemporary disagreements in economic thought leading up to what seems to be an impasse is excellent;[5] but, though he raises a number of pertinent questions, he does not seem to offer any very tangible answer. That the struggle between the classical and Austrian systems of economic thought was settling into a stalemate at the beginning of the twentieth century is clear enough. That Marshall in his eclecticism endeavored to reconcile conflicting ideas is

[4] Cf. especially *Quarterly Journal of Economics; Journal of Political Economy, Economic Journal, American Economic Review,* and *Annals of the American Academy of Political and Social Science;* see special references below.

[5] *Op. cit.*

likewise clear.[6] But that this eclecticism must be regarded for the most part as a snare rather than a solvent does not seem to be at all generally manifest even today. The system devised by Clark, and the critical utterances of Hobson, Veblen, and Mitchell, doubtless prevented economic theory from settling into an eclectic desuetude (it could hardly have been innocuous), and much of the criticism attacked the problem at its source. Nevertheless it still does not appear to be sufficiently understood that the real difficulties have to do with certain basic misconceptions implicit in both the classical and Austrian points of view, which an eclecticism rather serves to perpetuate than to expose. The way out would seem to be to clear up these misconceptions and pitfalls of reasoning, eliminate them from our Principles and other texts on economics, and proceed to lay down hypotheses in the spirit of exact science and in the light of twentieth century knowledge. It would appear to be about time that pseudo-science in economics were rooted out in all its forms.

2. The Alternative Use Doctrine

An example close at hand of the resurrection and renewed use of pseudo-scientific method in value theory is seen in the recent attempt to formulate a general law of price[7] by employing the so-called alternative-use or opportunity-lost doctrine, which in its essence dates back to Adam Smith, was later utilized by the Austrians, and received its more modern expression in the nineties at the hands of David I. Green of Johns Hopkins.[8] Forty years ago its fallaciousness would seem to have been most conclusively demonstrated in the searching criticisms of S. M. Macvane of Harvard,[9] yet it is apparently still very much

[6] But see A. C. Pigou (editor), *Memorials of Alfred Marshall*, Macmillan and Co., 1925, p. 418, letter of Marshall to J. B. Clark, March 24, 1908.

[7] Cf. F. H. Knight, "A Suggestion for Simplifying the Statement of the General Theory of Price," *Journal of Political Economy*, June, 1928, pp. 353–370; also, Edward S. Mason, "The Doctrine of Comparative Cost," *Quarterly Journal of Economics*, November, 1926, pp. 63–93; Raymond T. Bye, "The Nature and Fundamental Elements of Costs," *Quarterly Journal of Economics*, November, 1926, pp. 30–62; and A. E. Monroe, "Cost and its Relation to Value," *Quarterly Journal of Economics*, August, 1928, pp. 530–563. Monroe actually appears to define 'cost' as that which is 'given up' or 'foregone'—those alternative things we might have done—in entering upon any line of activity. Thus the 'cost' of a night's sleep would be the 'psychic income' we 'sacrifice' in not being able to spend the eight hours in doing something else. Among other writers who seem to favor similar ideas are H. J. Davenport, G. Cassel, F. A. Fetter, H. D. Henderson, T. N. Carver. (See Bye, *op. cit.*, p. 36).

[8] David I. Green, "Pain-Cost and Opportunity-Cost," *Quarterly Journal of Economics*, January, 1894, pp. 218–229.

[9] S. M. Macvane, "Marginal Utility and Value," *Quarterly Journal of Economics*,

alive, although dressed up today in a becoming modern style and making superficial use of the language of science.

The "alternative" formula is hailed in this recent literature as retaining "the scientific content of cost-of-production theory while sidetracking the sources of a century and a half of controversy";[10] as eliminating from general value discussion the treatment of "causes in contrast to laws"; as bringing to us the "only objective and scientific content in the cost notion",[11] the empirical law, "the scientific explanation of price";[12] as providing, with respect to utility, a similar scientific explanation, so that we may write, as an equation, cost equals price equals utility; and, finally, as making possible a considerable simplification in distribution theory, since, with the use of this formula, the factors of production "simply fail to put in an appearance."[13] Expanding the idea as applied to costs, it is held that, if these are stated "in terms of alternative commodities," all reference either to sacrifice or outlays may be omitted;[14] for, since labor effort can admittedly not be measured anyhow, either with respect to quantity, irksomeness, skill, or cooperation with the other factors of production,[15] many difficulties which have puzzled us hitherto are avoided. All one needs to do, to achieve these and other assumed beneficial results, is to state costs or utilities in terms of alternate products or uses, for it is the units that are allegedly "sacrificed in production" which measure these things.[16]

In the illustration here given, the alternative-use doctrine is presented with a rigor and consistency previously unequalled. Economists should be grateful for this forthright statement, even though the doctrine becomes more vulnerable in its nakedness, stripped of the usual equivocal encumbrances. To some extent throughout the whole period of economic writing, and certainly since the Austrians entered the arena of value theory, the method of stating costs and utilities in terms of alternative commodities—for example, that "the cost of beaver is deer and the cost of deer is beaver"[17]—has been pursued most vigorously. There has been lacking only the opportunity to meet

April, 1893, pp. 255 ff.; also, his articles "Böhm-Bawerk on Value and Wages," *Quarterly Journal of Economics*, October, 1890, pp. 24–43; and "The Austrian Theory of Value," *Annals of the American Academy of Political and Social Science*, November, 1893, pp. 348–377.

[10] Cf. Knight, *op. cit.*, pp. 355–356.

[11] *Ibid.*, p. 359.

[12] *Ibid.*, p. 360.

[13] *Ibid.*, p. 367.

[14] *Ibid.*, pp. 355–356.

[15] *Ibid.*, p. 357.

[16] *Ibid.*, p. 359.

[17] *Ibid.*, p. 359.

the issue squarely, and this is now provided in the recent literature, where, broadly stated, the doctrine would seem to be as follows: The cost, or utility, or price, or value, of any commodity is measured by the possible uses of its basic materials in other or alternate ways which had to be foregone in the production of the commodity in question; in short, by the cost, utility, price, or value, of other commodities lost or destroyed by virtue of its production.

It is well to note immediately, in the analysis of this doctrine, that it contains three logical parts: first, the device by which any one of the four terms mentioned is transferred from an *actual* to an *alternative* use; second, the method of relating one term to another to secure the aforementioned equation; third, the contention that alternative uses, or opportunities lost, affect in important ways the values of commodities. With the third part of this doctrine,[18] there can hardly be any legitimate quarrel. The significance of opposing demands in value theory would seem to need repeated emphasis. It is with the first two implications that we shall be concerned here.

(*a*). *The might-have-been method of measurement.*—To illustrate the meaning of the alternative-use assumption as a measure of cost, Macvane in his historic criticism takes as an example the use of wool in the making of coats, blankets, stockings, and other products.[19] The wool used in making any one of these products is lost to making the others, and this loss is presumed to measure the cost. Thus, if you wish to get at the cost of wool for making coats, you are told that it is the cost of wool for making blankets, carpets, etc. If you are seeking the cost of wool for making blankets, you are referred to carpets and coats. If you look for the cost of wool for carpet making, you are told to go to coats and blankets. The doctrine makes it necessary to look for the cost of a commodity in other places rather than in the commodity itself. Cost thus "insists on being something or belonging to something that might have been but is not. When you try to grasp it, to attach it to a real commodity, and to measure it as a definite, tangible quantity, it eludes you and retires to the region of the might-have-beens."[20]

An identical argument may be advanced with respect to utility.[21] It is merely necessary, wherever cost has been used in the foregoing illustration, to substitute utility with similar results. And the same reasoning applies to price and value.

But what is obtained by this might-have-been method? Is anything obtained at all? Say you have a stick about a yard in length. How

[18] Cf. Bye, *op. cit.*, p. 36 ff.
[19] Macvane, *Marginal Utility and Value*, p. 265.
[20] *Ibid.*, p. 269.
[21] Cf. Knight, *op. cit.*, p. 361.

measure it without some agreed-upon unit like the foot? Simple enough. Imagine another stick of exactly the same length and measure it thereby. Thus: Stick A is equal to imaginary stick B, which is all there is to it, and all bother about units and standards is eliminated.

Here we have, in brief, to what the first part of the alternative-use doctrine in economic theory would seem to come, whether applied to price, utility, value, or cost, or to any combination of these. In the matter of cost, it has been extremely difficult, apparently impossible in fact, to find an acceptable homogeneous unit of measurement. Labor pain, exertion, 'tired muscle and tedious waiting'—none of these is seemingly satisfactory. Since the actual problem is so disheartening, the shell game attempt to escape it was inaugurated by suggesting that the cost of commodity A is the cost of might-have-been commodity B is the cost of might-have-been commodity C. This procedure not only seems to have given no aid towards a solution, but to have kept the minds of economists from the actual problem. In trying to find out under which shell the cost or utility button has been put, some economists have been lulled into believing that they have been examining into the nature of the button itself. The might-have-been method focuses the eyes on the shells, the mind is kept occupied with guessing under which one the button is; hidden by this maneuver, the button itself never appears to have been laid bare for examination.

If the futility of this part of the alternative-use argument does not seem as yet sufficiently apparent, any attempt to develop by its means a general law of price (and this is what in recent literature appears to be the paramount issue) completes the exposure. How express, for example, the "cost of production of the whole group of commodities for which wool is a means of production?"[22] Not by referring wool to some additional alternative use, since in the broader assumption it has no other use. Shall we say that wool has a price, or utility, or cost, or value, with respect to this whole group of commodities, which is derived by anticipation from the commodities themselves? Apparently that, by assumption, is all we can legitimately state,—viz., the useless truism that the cost or price or value or utility of the whole group constitutes the cost or price or value or utility of the whole group! In endeavoring to formulate a general theory of price or value in terms of cost or utility on the basis of the alternative-use theory, there is no might-have-been alternative-use left to which one may be referred. No additional shell is in sight under which the cost or utility or price or value button might be concealed.

(*b*). *The 'equation' of cost-utility-price-value.*—The second part of the

[22] Macvane, *Marginal Utility and Value*, p. 266.

alternative-use doctrine, viz., the method of relating cost, utility, price, and value, one to another, would seem to be readily disposed of, once the significance of the first part is clear. The method is also indicated in Macvane's argument: "If the value of the means of production be the attributed value of the potential commodities contained in them, and if the value of the means constitutes the cost of the commodities, it seems to follow that cost, as a general phenomenon, is indistinguishable from value. If utility be the measure of cost, then is cost, as a general conception, identical with utility. It is robbed of all independent significance."[23] How is the equation 'cost equals price equals utility equals value' derived? Apparently by assuming these terms equivalent in the premises and then 'finding' them equivalent in the conclusion. If the value (or price) of the means constitutes the cost of the commodities, cost is the same as value (or price). If utility is the measure of cost, then cost is identical with utility. Hence by assumption all four terms are indistinguishable in the alternative doctrine.

This is not proof; it remains an unverified assumption. And its unverified character would doubtless be clear to everyone were it not for the hypnotic effect of the alternative-use shell game. The cost button is placed under one shell; by the alternative-use maneuver, it appears not only now under this shell and then under another, but occasionally the utility or the price or the value button is substituted for the cost button, creating the illusion that as a matter of fact the four buttons are identical. Destroy the shell game and the illusion goes with it.

Here is pseudo-science in economics. These putative formulae serve to confuse thought and to lull us into a feeling of accomplishment where there is none. Macvane's incisive and penetrating analysis of such sophistry can hardly be improved upon, although, when he himself comes to defend the classical cost position, his critics find him equally vulnerable.

3. Other Elements of Pseudo-Science in Cost and Utility Theory

Besides the 'alternative' dogma, which provides a flagrant example of pseudo-scientific method in economics, there are a number of other devices employed in the classical cost theories which it would seem legitimate to place in the same category. It may, e.g., now be confidently asserted that the early conception of a rude or natural society,[24] in which there was thought to be free exchange and equal com-

[23] Macvane, *Marginal Utility and Value*, pp. 267–268.

[24] Adam Smith, *Wealth of Nations*, Rogers edition, I, 49; also Von Wieser, *Natural Value*, 1893.

petition but no capital, and in which 'quantity of labor' determined cost, is not in any sense borne out by modern anthropological studies of primitive peoples.[25] Most of the classical structure built on this premise, therefore, would appear to fall to the ground; that is, whatever was left of it after the failure of the laborious attempt to arrive at a homogeneous measure of subjective cost[26] based upon the concepts of labor pain, labor effort, or 'tired muscle and tedious waiting.' With respect to current mathematical developments in price economics, one is tempted to suggest that the word 'expense' be uniformly used in place of 'cost' with its long heritage of subjective implications, even though the cost accountant might object to being called an expense accountant.

Faulty generalizations about early society, together with the 'alternative' sophistry, also helped materially in building up the structure of subjective utility analysis,[27] the most important form of which concerned itself with the supposed homogeneity and comparability of subjective utilities and disutilities, which modern psychology and C. E. Persons' penetrating analysis[28] appear to have finally made ready for the scrap heap, even though some economists here and there still drag out the rusty pieces and give them a new polish. The pseudo-scientific character of these concepts may be briefly summarized as follows: They assume a general rationality in human behavior which does not exist except in an exceedingly narrow sphere; they posit a refined system of barter which is quite out of harmony with the facts of the market; they substitute artificial abstractions (utility and disutility) for the actual pecuniary logic (profits and losses) which is at the bottom of a money economy; they continue to be based upon a purely hedonistic view of human nature, long since exploded, merely substituting utility-disutility for pleasure-pain with similar connotations and applications; their analogies (statics, normality, and the or-

[25] J. L. Myres, "The Beginnings of Science," in F. S. Marvin's *Science and Civilization*, Oxford, 1923, pp. 10–12.

[26] Cf. Green, *op. cit.;* also Monroe, *op. cit.*, Mason, *op. cit.*, Bye, *op. cit.*

[27] Cf. W. Smart, *An Introduction to the Theory of Value*, 3rd edition, 1914, especially his references to the views of Menger, Jevons, Wieser, and Böhm-Bawerk; also, J. B. Clark, "The Ultimate Standard of Value," *Yale Review*, 1892, pp. 258–274.

[28] Cf. C. E. Persons, "Marginal Utility and Marginal Disutility as Ultimate Standards of Value," *Quarterly Journal of Economics*, August, 1913, pp. 547–578. The author indicates that about 13 per cent of the people in a highly industrialized country receive half of the total income. His keen analysis of the influence of inequality of wealth upon utility theory should be sufficient to lay for all time the ghost of a homogeneous utility jelly, or of utility being "identical at the margin," or anywhere else, with disutility.

ganismic concept) are quite at variance with reality, the facts of evolution and institutional change, and modern researches in anthropology and sociology.[29]

4. Conclusion

While value theorists, trained in the old schools and hampered by an antiquated psychology and an unscientific attitude towards hypotheses, have been backing one another into an impasse, other economists have either refused to be drawn into the controversies, proceeding along mathematical lines to test out the implications of any given assumption and examining the price system as a thing apart, or have looked elsewhere for material out of which to build a foundation more in keeping with society and human nature as they actually are.[30] In these new endeavors, besides the significant mathematical contributions with which economists are becoming familiar, certain philosophers and sociologists have been and are continuing to be of considerable assistance (such men as Dewey, Perry, Stuart, and Cooley) and newer conceptions regarding value are taking form. In this basic field, one is beginning to hear less regarding classical dogmas and more about institutional settings and customary attitudes, about ever-changing interests and the impact of novel situations, total income and budgetary arrangements, price levels and purchasing power, trends and frequencies, stabilization and deliberate control, fair prices and moral judgments.[31]

In conclusion, then, I should like to present a twofold suggestion for consideration: First, that in the econometric approach, classical dogmas regarding value be abandoned and attention be focused upon a

[29] Cf. Thorstein Veblen, "The Limitations of Marginal Utility," *Journal of Political Economy*, XVII (1909), 620–636; W. C. Mitchell, "The Rationality of Economic Activity," *Journal of Political Economy*, XVIII (1910), 97–113, 197–216; E. H. Downey, "The Futility of Marginal Utility," *Journal of Political Economy*, XVIII (1910), 253–268; R. B. Perry, "Economic Value and Moral Value," *Quarterly Journal of Economics*, May, 1916, pp. 447–449; W. M. Adriance, "Specific Productivity," *Quarterly Journal of Economics*, XXIX (1914–15), 149–176; cf. also, references in footnote 31.

[30] Cf. references to Veblen and Mitchell in footnote 29.

[31] Cf. C. H. Cooley, "The Institutional Character of Pecuniary Valuation," *American Journal of Sociology*, 1912–13, pp. 543–555; J. A. Hobson, *Work and Wealth*, 1914, pp. 1–27, and *Free Thought in the Social Sciences*, 1926, pp. 61–166; W. C. Mitchell, "The Prospects of Economics," in Tugwell's *The Trend of Economics*, 1924, pp. 3–34; R. B. Perry, *op. cit.*, also *General Theory of Value*, 1926; John Laird, *The Idea of Value*, Cambridge University Press, 1929; H. W. Stuart, chapter on "The Phases of the Economic Interest" in *Creative Intelligence*, edited by John Dewey, 1917, pp. 310–340; A. A. Young, "Some Limitations of the Value Concept," *Quarterly Journal of Economics*, XXV (1911), 409–428.

thoroughgoing analysis of the objective price system and the internal congruity and logical implications of any economic theory which may be advanced; and, secondly, that, for the ultimate formulation of valid scientific hypotheses in the field of economics (hypotheses which will be to social science what the principles of the combination of elements are to chemistry or the theories of evolution and the cell are to biology), recourse be had more and more to the socio-psychological disciplines for basic materials regarding social institutions and human behavior.

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A REPLY

By Frank H. Knight

Being invited by the editor of Econometrica to make a short reply to Mr. Mayer's strictures, I am moved rather to come to his help. If there is any interest in price-theory economics from the standpoint of its limitations, I should like to see them more adequately set forth. The material is unrealistic, lacks finality as an explanation, involves circular reasoning, and is relative to problems and interests which change with the generations. (All of which applies to any explanatory or relevant statement whatever). Let us illustrate by an analogous piece of pseudo-science-sophistry from another field, that of medicine. Consider the statement, "quinine is a cure for malaria": (1) It gives no statistical information regarding any cures, the number, age, sex, occupation, etc., of the patients, or attendant circumstances. Shameful to behold, it does not even assert or unequivocally imply that there have been cures, but rather insinuates regarding them. (2) The statement certainly is not "true" as a generalization. Many people have malaria and do not know it, or take no interest in being cured, or haven't the knowledge or the price or the initiative, or try other treatments. Worse still, of those who have the disease and take quinine in some quantity and under some conditions many are not cured. (3) In the cure, when it does happen, inumerable other causes must cooperate. (4) Each of these has in turn its causes, and so on; the thing makes no pretence of getting down to ultimates. (5) If taking quinine causes the cure, the existence or fear of the malady causes the treatment—also the very production of the drug—a patent vicious circle. (6) The development of drainage and treatment of marshes for mosquitoes prevents malaria and there ceases to be any question of curing it.

Price-theory economics deals with economic behavior in somewhat the same way that medical pseudo-science-sophistry deals with "therapeutic" behavior. It has the same limitations in a much more pro-

nounced degree, notably because economic behavior has no partially definite objective, instrumentality, and procedure, corresponding to cure, medicine, and taking medicine, in the illustration. Assuming economic behavior to be a goal of action, as freedom from disease is a presumptive goal, it investigates one set of requisites for a "maximum" of want-satisfaction, namely, the organization factor, under given conditions as to other factors in the situation. That is, it deals with ends, means, and procedures (wants, goods and services and productive resources, and technology) "in general," the means being owned by individuals and utilized through a pecuniary exchange organization.

There is a better market for attacks on price-theory now than a century or half-century ago, because of waning enthusiasm for the social philosophy of individual liberty and exchange relations which sets its problem, the ideal of policy according to which each should use his own powers (including property—or property including personal powers) according to his own judgment to satisfy his own wants. Reasons for this change of attitude are evident, besides the fact that distance formerly lent enchantment to the view: great concentration of wealth, and greater concentration of economic power through new organization devices, and the growth of society to a size and complexity making it impossible for the ordinary individual to play a satisfactory rôle. Yet we cannot foresee a time when the fundamentals of the social order will make the more general conclusions of price theory inapplicable. This certainly would not happen under socialism or communism.

Of course I do not contend that any of this is "science," but merely remark that none of the limitations of price theory, nor all together, deprive it of truth and relevance. If truth and relevance have nothing to do with "science," I cheerfully admit that the theory in general, and my own paragraphs in particular, are not "science." The real conclusion of the paper, if not its major premise, is evidently that what is not "science" should not be discussed or studied and taught. With this I cannot agree, even for the sake of co-operating with Mr. Mayer.

In order to clarify the issue, I should like to ask one question: Assuming that wool is at hand, and has no other use than that of making its products, just what, speaking "scientifically" in the strictly orthodox and sacred sense, is the cost involved in using the wool to make those products? If no cost is involved, the *only* point to the entire harangue is to advocate in the name of "science" a prohibition law against any attempt to talk with relevance and truth about cost, and related concepts. If this is the point, it is harmless if made clear, and I trust that it is not the nature of "science" to put over prohibition laws by deception as to what its endeavor is.

The University of Chicago

A REJOINDER

By Joseph Mayer

For coming to my assistance in the suggestion of a medical analogy, I can only be grateful to Mr. Knight, though I want to assure him that I favor neither communism nor prohibition. We have had, forsooth, too many palliatives like quinine in value theory, when what we seem to need is a drainage of the marshes of economic thought to the end that malarial preconceptions may be done away with.

The relation of truth and relevance to empirical knowledge in the social field is so well expressed in a recent volume that I can hardly do better than refer to it and quote therefrom.[1] The minimum requirements of scientific method are there stated as follows: "That it shall employ techniques of observation and measurement designed to obtain data in terms of which a general proposition can be inductively established as possessing a definite degree of probability."[2] A proposition may be ever so 'true' when referred to a preconceived value premise, but, unless it possesses a fair degree of probability as representative of the actual world in which we live, it is quite irrelevant and useless so far as any analysis of economic behavior is concerned.[3] The title of my paper is ample warrant that I have no inclination to avoid or prohibit the discussion of that which is not scientific in value theory. My simple plea is that when discussing these matters we tag pseudo-science for what it is and stress the desirability of a truly scientific approach.

As for the question regarding 'cost,' the answer would seem to hinge upon whether subjective considerations (such as labor-pain, waiting, risk, etc.) are implied or objective features (such as entrepreneur's expenses or prices) are meant. Failure to keep these two meanings of 'cost' separated has been the cause of much of past misunderstanding in value theory. The alternative sophistry continues to confuse these two meanings and would appear to add nothing with respect to the relation (or lack of relation) of 'cost' to price.[4]

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[1] Jerome Michael and Mortimer J. Adler, *Crime, Law, and Social Science*, New York, 1933, pp. 56–64, 67, 71.

[2] *Ibid.*, pp. 63–64.

[3] Cf. also Joseph Mayer, "Towards a Science of Society," *American Journal of Sociology*, September, 1933.

[4] Cf. forthcoming book by present writer entitled *Pseudo-Scientific Economic Thought*, ch. II, on "Classical Cost Theory."

THE MEETING OF THE ECONOMETRIC SOCIETY IN CHICAGO, JUNE, 1933

A MEETING of the Econometric Society was held in Chicago, June 28-30, 1933, in connection with the summer sessions of the American Association for the Advancement of Science. An important feature was the sessions on the afternoon and evening of June 30, held jointly with the American Society of Mechanical Engineers, the American Society for Testing Materials, and the American Institute of Electrical Engineers. These joint meetings were attended by more than 200 engineers, many of whom manifested a keen interest in the aims of the Econometric Society.

On Wednesday, June 28, at 10.00 A.M., in the Social Science Assembly Room, Social Science Building, University of Chicago, there was a joint session of the Econometric Society and Section K of the American Association for the Advancement of Science. The general subject was "Elasticities of Demand and Supply." Colonel Edward N. Wentworth, of Armours' Livestock Bureau, acted as Chairman.

Sir Daniel Hall, Director of the John Innes Horticultural Institution, of Merton, England, spoke on "Elasticities of Demand for Agricultural Products." Among other interesting things, he discussed the relation between employment in agricultural lines and the shifts in demand on the part of the public from less processed to more processed agricultural commodities, as from cereals to meats.

Professor Harry Pelle Hartkemeier, of the University of Missouri, spoke on "Elasticity of Supply for Agricultural Products, with Special Application to Corn and Potatoes."[1] Professor Hartkemeier said, "Before the elasticity of supply can be measured, it is necessary to determine the supply curve or function for each agricultural product. This is generally more difficult than the determination of the demand curve, for weather fluctuations interfere with the attempts of the farmers to adjust production to price changes. The method of multiple correlation was applied to the data for production, price (money and deflated), time, temperature, and precipitation, to secure the supply functions for potatoes and corn during the period 1896-1914. It is interesting to note that the farmers producing potatoes not only planned their production on the basis of the price of the preceding year but they also made allowance for the price changes caused by abnor-

[1] This paper presented only a part of the methods and results which will be published shortly as "The Supply Function for Agricultural Commodities. A Study of the Effect of Weather and Price on the Production of Potatoes and Corn." *University of Missouri Studies*, VII, Mo. 4.

mal weather conditions. This was proved statistically. The elasticity of supply for potatoes decreased throughout the period, for the supply curve shifted its position. The farmers gradually became more willing to produce a larger quantity of potatoes at the same price, or the same quantity at a lower price. The reaction of the production of corn to the price of the preceding year could not be established, for the slope of the supply curve was not significantly different from zero. Consequently, the coefficient of the elasticity of supply was zero. This difference between potatoes and corn is caused by several factors which have an important bearing upon the plans of the federal government to control the production and price of agricultural commodities."

In speaking on "Elasticity of Demand for Producers' Goods as Illustrated by the Demand for Iron and Steel," Dr. Roswell Whitman, of the Illinois Emergency Relief Commission, noted that the complexity of their market had made the study of the demand for producers' goods more difficult than that for agricultural commodities. The statistical techniques for the two studies had been similar, but for producers' goods it had been necessary to develop a new demand theory. The older theory failed to account for the highly fluctuating markets for such commodities. The Evans equation,

$$y = ap + b + h\frac{dp}{dt},$$

expressing demand as a function of price and the rate of change of price, had been of particular importance in the theoretical development. Dr. Whitman reported that the first tentative analysis of the demand for steel indicated that when such factors as business conditions were taken into account by statistical methods, sales of steel varied directly with rising and falling prices, but were larger when the price was low and smaller when it was high. This last result solved the enigma of Moore's "positively sloping demand curve" for a producers' good. The qualitative results from this study were satisfactory, Dr. Whitman said, but the significance of the coefficients obtained was limited by the use of the method of multiple correlation for the analysis of time series. Further tests and wider application of the method were necessary, but the speaker concluded that at least the study opened a promising field in price analysis.

The final paper of the symposium was on "The Elasticy of Demand for Cotton," by L. H. Bean of the U. S. Department of Agriculture.

For the practicing statistician and economist working on problems relating to cotton, Mr. Bean stated, certain demand schedules were of practical importance, particularly in determining national and individual acreage policies, in efforts to remove an enormous surplus created

by the depression, and to regulate the volume of consumption at home and exports to foreign markets. Among the important demand schedules were the relation of domestic mill consumption of cotton and price and the relation of total supply of American cotton and price. The first revealed the elasticities of demand for that part of the total supply converted by mills for industrial and other uses in the domestic market. The other revealed elasticities of the composite demand, covering not only domestic mill demand but also the carryover and export demand. Both of these schedules, Mr. Bean said, indicated relatively inelastic demand, particularly at relatively high prices. The demand schedule dealing with domestic consumption was shown to have shifted vertically during the post-war period, with the rise and fall in total world consumption, the latter representing changes in world industrial conditions. In the derivation of these two demand schedules, Mr. Bean used the simplified method of curvilinear correlation. He held that this method was used to great advantage in practical work, for it revealed the nature of the net regressions involved without the necessity of fitting arbitrarily selected functions, and without any of the usual machine calculations. Once the net regressions had been determined, their description by equations might be undertaken, if desired. In the two examples in Mr. Bean's paper, the validity of the results was indicated, not by means of coefficients of correlation and standard errors, but by the more significant fact, in time series analyses, that relations established on the basis of observations for a part of the post-war period continued to hold good even during the period of drastic change, 1929-1932.

An informal meeting was held on the afternoon of the same day for the discussion of the subjects presented in the morning session, this discussion being led by Professor Harold Hotelling of Columbia University and Professor Henry Schultz of the University of Chicago.

The second symposium, on "Business Cycle Theory," was held on Thursday, June 29, at 10.00 A.M. in a joint session of the Econometric Society and Section K, A.A.A.S., in the Social Science Assembly Room, Social Science Building, University of Chicago. Professor Wesley C. Mitchell, of Columbia University, presided.

Dr. Carl Snyder, of the Federal Reserve Bank of New York, opened the session with a paper on "Real Income and Real Wages From 1790." Dr. Snyder said, "A long series of studies, extending now over the last thirteen years, has made possible fair estimates of the rate of industrial growth of the United States from 1790, including in these estimates not merely basic production but the total of goods and services of all kinds. When these measures are reduced to a per capita basis, they disclose a nearly unchanging rate of growth from the earliest period,

averaging between 2½ and 3 per cent per annum. Part of this has been due to the remarkable change in the age-character of the population, the percentage of adult population, i.e., those of twenty years or over, rising from 40 percent in 1800 to 60 percent at the present time. But the chief factor has been the immense growth of capital invested in manufacturing, transportation, and the like, which has been steadily at a higher rate than the increase of the industrial product. This colossal gain in capital (easily exceeding a hundred billions of dollars) has been derived largely from the savings of the industries themselves, and not from individual savings, which are relatively small. Correspondingly, there has been a long-time, but not nearly so regular, increase in 'real' wages, averaging, per worker, through a century and more, around 1 per cent per annum compounded. When the increase of the proportion of the population gainfully employed is allowed for, this means that the workers as a whole have absorbed a considerable share of the gain in the industrial product, the balance going to capital account or 'savings.' Without this latter, the astonishing advance of the period would have been impossible."

The second paper of the symposium, by Professor Harold T. Davis of Indiana University and The Cowles Commission for Research in Economics, was devoted to "Contributions to the Theory of the Harmonic Analysis of Economic Series."

Professor Davis said that the problem of long-range forecasting of the behavior of economic series, such as price and stock market indexes, depended upon the possibility of forecasting general trends on the one hand, and upon the determination of persistent movements on the other. With a view to answering the question whether genuine cyclical movements exist in economic series, the Cowles Commission for Research in Economics had harmonically analyzed (in the sense of the Schuster periodogram) five series, each extending over a 100-year period, and had also analyzed each of these 100-year series by 25-year intervals. The following series had been included in the analysis: Rail Bond Prices, Business Activity, Wholesale Prices, Rail Stock Prices, and Commercial Paper Rates. Alfred Cowles 3rd, in a recent paper (ECONOMETRICA, I, 309-324), had pointed out the failure of professional forecasters to predict movements of the stock market. Their prediction, as a matter of fact, had been somewhat worse than that resulting from completely random prediction. This conclusion led one to inquire into the extent that cyclical movements discovered by periodogram analysis were random phenomena. The investigation was thus directed toward an analysis of random series. A significant fact was noted in the similarity between the auto-correlation diagrams of economic series and those of random series which have been

smoothed by a λ-month moving average. This similarity was apparently due to the fact that month-by-month changes in economic series were correlated approximately in the same manner as the elements of the smoothed random series, each term of which contained λ-1 elements of the preceding.

Employing a theorem of N. Wiener (*Acta Mathematica* LV [1930] 117-258), it was found that when a random series was smoothed by means of a λ-month moving average, the smoothed series would have periods at .6823λ and 2.6954λ. This theorem was due originally to R. Frisch, who, however, employing other means, found the second period at 3.2λ. An estimate of the value of λ necessary to construct any given economic series, so far as its periodicity was concerned, from a random series, could be obtained from the first zero-point on the auto-correlation graph. Since λ for many series was close to 12, this led to the conclusion that periods were to be found around 8 and 30 months, the latter being the more pronounced. The period-range from 25 to 35 months was found to be particularly rich in major periods in the economic series which were studied.

The period-range from 40 to 50 months was also found to be rich in periods, which could not be explained on the basis of random series smoothed by moving averages. A period of approximately 40 months was found in the Dow Jones industrial stock averages (1897 to 1913), and in the business activity series and rail stock prices for the period from 1831 to 1931. Its right to be regarded as a persistent characteristic of some economic series was argued for in the fact that it tended to appear in the 25-year periods obtained by breaking up these two 100-year series into four parts.

In the discussion of Professor Davis's paper, Professor Harold Hotelling of Columbia University commented that, of the various periodogram analyses of economic data, Professor Davis's showed by far the greatest mathematical ingenuity and skill. Professor Hotelling said further that the question might be raised as to whether analysis of economic series into periodic components by any method was significant. From Professor Fisher's paper and similar considerations he thought we might suspect that each crisis was to be regarded as a distinctive event, with its own oscillations, which were not part of a long-continuing oscillation embracing them all. In these circumstances, harmonic analysis of a long economic series resembled harmonic analysis of a man's temperature since his birth. There would be a sharp increase and decrease, with possible oscillations, each time he got a disease; but these would not combine seriatim to give something discernible by means of any of the periodograms. "But if we do make periodogram studies," Professor Hotelling went on, "there is need for

emphasizing the same principle as in other statistical research: we *must* check the results by interpreting them in terms of probability. This requirement gives a strong argument for the modified Schuster periodogram method given by R. A. Fisher in the *Proceedings of the Royal Society* for 1929. This, incidentally, is not the same as the periodogram of Whittaker and Robinson, which seems to be a much less efficient scheme, having larger probable errors. The method of Felix Bernstein, with the extension in Hans Münzner's Göttingen thesis of 1932, also deserves examination. I must take issue with the objection to the use of multiple correlation and regression in economics. It seems to me that the work of Henry Schultz, for example is of the greatest importance. If the determinant of the normal equations seems close to zero in a particular instance, this fact merely implies that we have little information regarding the parameters whose values are being sought. This lack of information is fully taken account of in the standard errors of the unknowns, when these are used in accordance with *modern* statistical theory—by which I mean that Student's distribution, with the appropriate number of degrees of freedom, is used instead of the normal distribution in the interpretation of the standard errors."

The next address of the session, on "The Monetary Factor in Business Cycles," was given by Dr. N. J. Silberling of the Silberling Research Corporation, San Francisco. The speaker held that the so-called Equation of Exchange, as it has been mathematically formulated in authoritative discussions, had given rise to theoretical conclusions which appeared to emphasize unduly the money and credit variables as dominating influences in creating cyclical fluctuations in prices and business. He thought that if we conceived of the effective money and credit factors as the average turnover, through credit operations of a basic monetary stock consisting ordinarily mainly of gold, we might consistently set up on the right side of the Equation a correlative magnitude of supply of goods and services which had its appropriate average rate of turnover, the product of supply and turnover being further multiplied by an index of price change. This would bring definitely into the equation the supply of goods, equipment, buildings, organized business units, and potential services. The usual reference to trade (T) in the Equation, naturally removed from the right-hand of the Equation one of the great motivating factors in prices, since T is not concerned with supplies, in spite of the fact that the left-hand side of the Equation contains a supply of money and/or credit. Dr. Silberling thought that by investigating the effect of supply upon price, it would be found that the changes in the price level could be in large part explained through unbalanced changes in sup-

plies, particularly of capital goods in relation to consumers' goods. In the initiation of cyclical fluctuations the supply factor might operate in advance of, and as a determining factor in, credit, and the speaker said recent events appeared to support this contention. The proposals for managed currencies, easy money rates, and various forms of monetary stabilization, all appeared to rest on an illogical Equation of Exchange and erroneous conclusions drawn therefrom, and such proposals had intensified and prolonged the depression. So long as we experimented with credit or monetary controls which are powerless in themselves to restore a balance between one group of supply factors and another, we merely added further supply to an unbalanced condition, and made a normal recovery of the price level impossible. The present depression, complicated as it had been by international difficulties, obsolete political interference, declining rate of population growth, and unfortunate fiscal blunders, seemed none the less to be mainly intensified by wrongly conceived experiments designed to make our credit system a means of controlling what in the very nature of things it cannot control.

Professor Irving Fisher, of Yale University, next spoke on "The Debt-Deflation Theory of Great Depressions." Professor Fisher summarized the conclusions on cycle theory reached in his *Booms and Depressions* and other writings, as well as the conclusions of other writers which he accepted. He gave his tentative "creed" on the subject under 49 articles. His principle contention, which he believed to be original, was that the present depression, and most other great depressions, were due to too much debt, which, by distress selling and liquidating bank loans (thus using up much of the deposit currency of the country) led to deflation of the price level, and that this deflation proceeded faster than the liquidation which caused it. The result was that the debt remaining unpaid, while nominally reduced, really increased, as measured in commodities. While the number of dollars owed decreased, the value of each dollar owed increased still faster. That is, the very effort to lighten the debt burden actually made it heavier. This made an unstable situation, and tended to make the depression grow worse by the very efforts to make it better. Under these conditions, if the remedy were sought in further liquidation, the depression would not end until bankruptcy and unemployment were well-nigh universal. Professor Fisher concluded that the better and quicker way out was via" reflation," such as had been approved by President Roosevelt.

Professor James G. Smith, of Princeton University, presented the concluding paper of the session, "The Impact of Business Cycle Theory upon Political Thought and Action." "The emphasis of disequilibrium

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forces in the economic order by business cycle theorists," said Professor Smith, "has been accompanied by the fall into disrepute of the equilibrium theories of the classical economists. The effect upon political thought and action has been the ascendency of a social-economic philosophy which may be summarized in the term 'economic planning,' formerly championed only by minority political groups. The development of the new psychological economic doctrines, particularly of the American school of thought following and perfecting the Austrian theories, leads to a new general equilibrium concept. The new general equilibrium concept is dynamic and realistic. The economic order is not 'planless.' The events of the period since 1929 are a demonstration of the existence of equilibrating forces made necessary by futile economic planning; and they are not a demonstration of the existence of inherent disequilibria, as is now commonly argued. Consideration of (1) inherent forces towards disequilibrium and (2) positive or non-inherent forces, shows that the explanation of the business cycle lies in the analysis of the positive or non-inherent forces and not the inherent forces. The effect of such a business cycle theory as advanced in this paper upon political thought and action is explained. Competitive capitalism is not a product of laissez-faire. The period from 1920 to the present is an era of economic planning, with disastrous results. The mechanistic 'system analysis' of the business cycle leads to futile and often harmful panaceas. Society is not a robot."

On Friday afternoon, June 30, at 2:00 o'clock, a joint session of the Econometric Society, the American Society of Mechanical Engineers, the American Society for Testing Materials, and the American Institute of Electrical Engineers, was held in Room 405 of the Palmer House. Professor Irving Fisher presided. The general subject of the symposium was "Some Fundamental Problems of Mutual Interest to Scientific Economists and Engineers."

The first paper, "Contributions of the Mathematician to Economics," by Dr. Charles F. Roos, was read by Professor Harold T. Davis in the absence of Dr. Roos. This paper touched lightly upon some of the early uses of mathematics in economics, but it was chiefly devoted to a study of the effects of past prices, present prices, and expected future prices, upon demand. It was stated that the effects of these quantities could be best considered by using the theory of integral equations—since the very definition of an integral makes it a function of all points of a curve—for the demand problem, the curve represented by price as a function of time. Thus, it was shown that proper demand relations are functionals in form. In the simplest case, that is, the one for which the demand relation is an integral, the statements (1) demand depends upon past prices, and (2) price depends upon past demands, were shown

to be entirely equivalent because of the invertibility of the integral equation. Present demand was assumed to consist of (a) consummations of past decisions to purchase as soon as funds become available, (b) climaxes of growing desires to purchase, and (c) purchases that are called forth chiefly by the present price of the goods. For (a) many will make their purchases within a relatively short time, and some will never be able to purchase. This situation may be typified by assuming that the factors affecting the periods of time required for the transformation of desires to purchase created at time t_1 into actual purchases at t are such that decisions to purchase are transformed into purchases according to a Pearsonian Type III frequency function. For this function and certain other somewhat similar ones, it was shown that the continuous weight function for past prices is of the form $f(x)K(x-t)$ where x refers to past time and t to present time. A typical form of weight function for (*b*) was also suggested. It was further assumed that speculative demand depends upon expected future prices and shown that the demand law for the common situation of predicting prices one unit in advance of t consists of a function of p, a functional of p to represent past influences, and an expression involving the derivative of p with respect to t. A short discussion was given of time lags and a functional elasticity of demand was defined.

In discussing Dr. Roos's paper, Professor Henry Schultz of the University of Chicago commented on the fact that it improved and generalized Roos's earlier theories in several particulars. Since it allowed for the effect of past and future, as well as present, prices on demand, it was possible, under certain conditions, to write the mathematical functions which showed the way equilibrium price and equilibrium quantity changed with time. Since these functions might contain periodic terms, they suggested the possibility of constructing a link between pure theory and business cycle phenomena. Professor Schultz further stressed the importance of statistical studies to determine to what extent these newer theories of demand explained the market behavior of consumers and traders. In conclusion, Dr. Schultz indicated ways in which a knowledge of contemporary economic research might be of value to business men, and paid tribute to the contribution of engineering to economics through such men as Walras, Pareto, Dupuit, Colson, Divisia, and Ellet.

Dr. Thornton C. Fry, of the Bell Telephone Laboratories, then spoke on "A Mathematical Theory of Rational Inference." He first discussed the bearing of Bayes's Theorem upon the problem of rational inference, pointing out that, if mathematical probability be regarded as an index to the degree of uncertainty with which a rational inference is surrounded, then Bayes's Theorem leads to the following subsidiary

propositions regarding the use of data as a means of testing the acceptability of hypotheses: (a) in the formation of a rational inference, both the tangible statistical data and the intangible collateral information must be weighed; (b) when data and collateral information are contradictory, the data must be more conclusive than would otherwise be required; (c) statistical data which are equally consistent with two hypotheses are worthless as a means of differentiating between them; (d) so far as the collateral information differs from man to man, the degree of assurance warranted by any given experiment must also be a personal matter. From these considerations two conclusions were drawn: that a sound statistical theory must confine itself to the implications of the tangible experimental data which is common to all, and avoid common sense considerations (such as "weighting" data) which are necessarily personal; and that its results should be expressed in forms that will make it as easy as possible for the ultimate user to give reasonable weight to the intangible factors.

In discussing Dr. Fry's paper, Dr. Anson Hayes of the American Rolling Mills Company noted that it had an important and practical bearing on the work of the American Society for Testing Materials. That Society was incorporated for the purpose of promoting knowledge of engineering materials, and the standardization of specifications and methods of testing. Such work involved at every turn the collection and utilization of vast quantities of technical data. Such data formed an important part of the source material used in arriving at standards. These standards must be satisfactory to both the consumer and the producer. One of the most important problems was that of interpreting the experimental data thus obtained. Interpretation consists essentially in formulating judgments based on the immediate data, and also on all of the relevant collateral information. These judgments were invariably probability judgments, as Dr. Fry had indicated, and the conclusions reached were therefore never *certain*. Dr. Hayes stressed the importance of two considerations; first, that the philosophy of probable inference was one of the fundamental tools that must be used if we were to attain an objective of choosing quality standards that are *economic*, and second, to attain this objective, there should be, in each branch of industry interested in standardization, a few men, highly trained in the philosophy of probable inference, who could aid in the development of techniques for establishing and maintaining economic standards of quality.

In further discussion of Dr. Fry's paper, Professor Harold Hotelling of Columbia University called attention to certain limitations of the method of inverse probability as presented, and also to available substitute methods. "A distinction must be made," said Pro-

fessor Hotelling, "between probability in the loose generic sense of degree of rational belief, and on the other hand, mathematical or objective probability, which is taken to be a limiting ratio of occurrences of a particular type. The former, which may be called subjective probability, not as a term of reproach but to make clear the distinction, includes objective probability as a particular case of greatest importance. But it must not be supposed that every subjective probability, in the sense of a relation between a set of hypotheses and an uncertain conclusion, can be assimilated to a number between zero and one. This supposition leads to absurd conclusions, as Keynes showed. Hence the *a priori* probabilities which enter into the formula of inverse probabilities cannot be taken as numerical quantities in general; consequently the symbols obtained from the formula cannot in general be interpreted as numbers. This limits the apparent usefulness of the formula a great deal. It cannot be used to estimate objective probabilities, or to serve as a basis for action with the reasonable expectation of success in the calculated proportion of cases, save in the rare and trivial case of definite knowledge of the *a priori* probabilities. Other measures of degree of rational belief are available by means of likelihood, and by means also of tests of significance, which form the really substantial part of the modern development of the theory of probable inference."

Mr. L. K. Sillcox, of the New York Air Brake Company, concluded the discussion of Dr. Fry's paper. He said, in part, "An engineer attempts to make use of physical properties and physical laws in the fabrication of products to satisfy human wants. It is generally assumed that this should be done economically, having in mind the consumption of a minimum amount of effort and a minimum cost of material. Obviously, the engineer needs to have knowledge as to what can be done physically and as to what people want, subject to an assumed economic system. Specifically, in the field of natural science, one needs to know, first, physical constants and distribution functions representing physical property, second, laws of relationship between properties, both functional and statistical, and last, causal relationships. Similarly, the engineer needs to discover social facts, laws, and causes, in so far as they can be reduced to measurements. It is well recognized, of course, that any judgment of inference based on observed data must be in the nature of probable inferences, and it is in this connection that the discussion of Dr. Fry's paper is of particular importance, as indicating one aspect of the philosophical background underlying the approach of probability theories. It is important, however, that we should keep in mind the importance of rational probable inferences as not being limited to the field of induction, so important in the interpretation

of physical and social data, but also in the field of deduction, so important in the design of equipment and the establishment of efficient, reliable, and economical inspection methods in the process of employing our judgments in respect to physical and social facts, laws, and causes. The practical field of application of statistical theory in the establishment of distribution of tolerances on piece parts or quality characteristics resides in the fact that they must come within the overall allowable tolerances. Two types of approach appear from this view-point, one being the overall quality as a function of the qualities of component parts in such a way that the resultant deviation is of a positive kind, whereas in the other one the resultant deviation is of a cycle or chain type with respect to the component parts. It is well to emphasize that the inductive probability inference in design and development affect economies in engineering work. Most of us realize the practical significance of the studies in the mathematics and philosophies involved in large proportions in engineering work, and it is indeed a hopeful sign that committees have now been established in America, England, and Germany, to co-operate in the establishment of more efficient engineering practices, both in the field of discovery and use of engineering and social data. I wish to emphasize the need for technically trained men in the engineering industry to handle the problems of rational probable inferences. The complete theory is quite useless unless offered as a highly developed tool in the hands of a competent workman. There are at least two general methods in use by engineers in arriving at satisfactory conclusions to controversial problems involving quantity measurements. One is for those charged with the responsibility to agree upon the interpretation of facts as presented and pertaining to the whole mass, whereas the other is for those interested to agree to a method of measuring the separate units, thereby obtaining as many sets of numbers as there are units, and further, to agree to a method of comparing the sets of numbers so obtained. In the end, we know that the specification of a standard is of value only in so far as it indicates a way of arriving at an empirical decision acceptable to all concerned, as, for instance, to both producer and consumer considered on the basis of a given standard of quality."

The concluding address of the afternoon session, on the subject "The Engineering Economist of the Future," was given by Dean Dexter S. Kimball of Cornell University. He referred to the confusion of present economic thought and its failure to predict, which rob it of the character of a "science." "Engineers have progressed," said Dr. Kimball,"beyond their first empirical principles of production till today the economic principles of production and their limitations are quite fully determined. Advances in transportation and communi-

cation have made an economic system as sensitive as it is complex, and our system of distribution calls for immediate remedies. The engineer's usefulness in this field of industrial management has been widely attested, and it is in this field that the greatest reforms are needed, in finance, transportation, and self-government in industry. In the broader field of 'political economy,' however, the status of the engineer is more uncertain, for only occasionally can he apply the same methods that were so successful in production problems. In general economics, as distinguished from engineering, the evidence may be voluminous, the variables many and conflicting and, what is worse, changing in character. The engineer has no advantage here over the lawyer or business man. The engineer, to figure importantly in public affairs, must acquire a broader technique and a wider variety of information, a knowledge of economic history and an ability to trace the effect of economic change over long periods of time. He must expect to do an unusual amount of studying. He must avoid dogma, for economic variables change rapidly. The engineer must take thought of the limitations of his methods as he ventures into the troubled domain of economics."

In the discussion which ensued, Mr. C. F. Hirshfeld of the Detroit Edison Company echoed Dr. Kimball's warning against engineers' too hastily attempting to settle out of hand economic problems which have for generations vexed the best brains in the world. "The confusion in economics, however," he said, " is no worse than was that in physics, chemistry, and biology, a few decades back. Indeed, after advancing beyond the elementary concepts, there is quite a bit of confusion in these sciences today. A more rigorous definition of economic terms and concepts is called for, as well as thinking and reasoning within the terms of these concepts. Mathematics forwards this type of reasoning. The engineer brings to economic problems the true scientific method, but his efforts will prove useless unless they are supplemented by intensive study of the forces peculiar to the economic situation."

President Harvey N. Davis of the Stevens Institute of Technology was inclined to the belief that Dean Kimball erred on the side of caution in warning the engineer against pitfalls. He stated that the engineer, in that he is quite accustomed to dealing with dynamical concepts, might make a very important contribution to the development of economics.

On Friday evening, June 30, at 8:00 o'clock, in the Red Lacquer Room of the Palmer House, there was a second joint session of the Econometric Society and the American Society of Mechanical Engineers, the American Society for Testing Materials, and the American Institute of Electrical Engineers. Mr. Ralph E. Flanders, Chairman of

the Committee on Economic Balance of the American Engineering Council, presided.

In a paper, "Some Fundamental Problems of the Engineer," Dr. F. B. Jewett, president of the Bell Telephone Laboratories, gave a general picture of some of the things a social scientist needs to know about the present and potential possibilities of engineering developments, and some of those that the research and development engineer interested in raising the standards of living needs to know about the present and potential possibilities of social and economic developments, if each is to cooperate with the other in the most successful solution of many of the complex problems of mutual interest.

The second address of the evening, on "The Internationalization of Scientific Knowledge as a Factor in World Economic Recovery," was given by Mr. A. P. M. Fleming of the Metropolitan-Vickers Electrical Company, Manchester, England. "There are great economic possibilities," said Mr. Fleming, "in internationally concentrating and disseminating scientific knowledge and experience, the subject deriving its importance from its bearing on the social-economic problem of unemployment which is world-wide except for Russia, and from the imminence of the challenge to the individualistic and capitalistic systems of the rest of the world by the Russian commercial system. Since the distribution of natural resources bears no relation to political boundaries, raw materials link the world internationally. Inventions and discoveries finally take on an international character. Unfortunately these facts do not lead to full international cooperation. Scientific research is directed by competitive conditions to effect improvements which create unemployment. Much of this research effort should be aimed at development of new lines of industrial activity. Russia is possessed of vast natural resources, and its 200 component nationalities are already internationalized. The Soviet Union supports a larger body of organized research workers under unified control than any other country. Only co-operation in the joint use of scientific resources will enable the rest of the world to meet the Russian challenge successfully."

ALFRED COWLES, 3RD.

LIST OF FELLOWS OF THE ECONOMETRIC SOCIETY

THE Constitution of the Econometric Society defines the qualifications, functions, and elections of Fellows in Section 3, as follows:

"All Fellows of the Society shall be nominated by the Council and elected by mail-vote of the Fellows. Such nomination may be made at any time. To be eligible for such nomination a person must have published original contributions to economic theory, or to such statistical, mathematical, or accounting analyses as have a definite bearing on problems in economic theory, and must have been a member of the Society for at least one year. Each year the Council shall offer the members an opportunity to suggest nominees for fellowships.

The first group of Fellows shall be elected by the Council.

A meeting of the Fellows shall be held each year at a place and time designated at the previous annual meeting of the Fellows. In default of action by the Fellows, the Council shall have the power to fix the place and date of such meeting. The annual meeting of the Fellows shall represent the highest authority of the Society. In important cases, the annual meeting of the Fellows may decide by a simple majority vote to subject any decision of the meeting to ratification by mail-vote of all Fellows.

In accordance with the provisions of the Constitution, the first body of Fellows has been elected by vote of the Council of the Society. The Fellows elected are: Professor Luigi Amoroso, Rome; Professor Oskar N. Anderson, Varna; Dr. Albert Aupetit, Paris; Professor Boninsegni, Lausanne; Professor A. L. Bowley, London; Professor Clément Colson, Paris; Professor Gustavo Del Vecchio, Bologna; Professor François Divisia, Paris; Professor Griffith C. Evans, Houston; Professor Irving Fisher, New Haven; Professor Ragnar Frisch, Oslo; Professor Corrado Gini, Rome; Dr. Gottfried Haberler, Vienna; Professor Harold Hotelling, New York; Professor John M. Keynes, Cambridge; Dr. N. D. Kondratieff, Russia; Professor Wesley C. Mitchell, New York; Professor H. L. Moore, Cornwall, N.Y.; Professor Umberto Ricci, Giza; Dr. Charles F. Roos, Washington; M. Jacques Rueff, London; Dr. Erich Schneider, Dortmund; Professor Henry Schultz, Chicago; Professor Joseph A. Schumpeter, Cambridge, Mass.; Dr. J. Tinbergen, Scheveningen; Professor Felice Vinci, Bologna; Professor Edwin B. Wilson, Boston; Professor Wl. Zawadzki, Warsaw; Professor F. Zeuthen, Copenhagen.

IRVING FISHER
Chairman of the Council

PRELIMINARY PROGRAMS OF THE MEETINGS OF THE ECONOMETRIC SOCIETY IN PHILADELPHIA AND BOSTON, DECEMBER 27–30, 1933

THE Econometric Society will meet with the Social Science Societies in Philadelphia on December 27–29, 1933. The headquarters and location of all meetings of the Society will be at the Benjamin Franklin Hotel, on Ninth Street between Sansom and Chestnut Streets. The minimum rates for single rooms are $3.50 per day, and for double rooms $5.00. For those preferring to stay near the University of Pennsylvania, the Hotel Pennsylvania, Chestnut Street at 39th, has single rooms at a minimum rate of $3.00 and double rooms at $5.00. Professor J. Shohat of the University of Pennsylvania is in charge of local arrangements. Those who plan to attend are reminded that special reduced rates are granted by the railroads.

The chairman of the program committee is Dr. Charles F. Roos, 3833 Garfield St., N.W., Washington, D. C. The other members of this committee are Professors Irving Fisher, Joseph A. Schumpeter, Edwin B. Wilson, and Dr. Walter A. Shewhart.

On the morning of December 27, the American Statistical Association will join with the Econometric Society for two sessions on "Statistical Work of Government Bureaus." At a session beginning at 9:00 A.M., papers will be presented by Dr. Spurgeon Bell of the National Recovery Administration, and by Dr. Corrington Gill of the Emergency Relief Corporation. This session is expected to adjourn at 10:30 A.M. At 11:00 A.M., a second joint session will be held. Papers will be presented by Dr. Stuart Rice, Assistant Director of the United States Census Bureau, Dr. Isador Lubin, United States Commissioner of Labor Statistics, Dr. William Riefler, Chairman of the Central Statistical Committee of the United States Government and Director of Research for the Federal Reserve Board, and Dr. Mordecai Ezekiel, Economic Adviser to the Secretary of Agriculture.

On December 27, at 3:00 P.M., a session will be held on "The Time Element and Economic Theory." Papers will be presented by Dr. Charles F. Roos of the National Recovery Administration, Professor Griffith C. Evans of the Rice Institute, and others.

On December 27, at 8:30 P.M., Dr. Alexander Sachs, Chief of the Division of Economic Planning and Research of the National Recovery Administration, will deliver a public address on "The Postulates of Economic Planning."

On December 28, at 10:00 A.M., there will be a session on "Price, Supply, and Demand Phenomena," among those participating being

Mr. Elmer J. Working of the United States Bureau of Agricultural Economics, Professor F. A. Pearson of Cornell University, and Dr. Elizabeth Gilboy of the Harvard Committee on Economic Research.

The American Statistical Association will also join with the Econometric Society for a sesson at 3:00 P.M. on December 28, the topic being "The Statistical Analysis of Time Series." Among those participating will be Professor Harold Hotelling of Columbia University, Professor H. T. Davis of the University of Indiana, Mr. Herbert Harvey of the Lehman Corporation, and Mr. Max Sasuly and Mr. Victor von Szeliski of the National Recovery Administration.

On December 28, at 8:30 P.M., Dr. Mordecai Ezekiel, Economic Adviser to the Secretary of Agriculture, will deliver a public address on "Problems of Agricultural Planning."

The closing session will be on "Economic Forecasting" and will be held on December 29 at 9:30 A.M. Among those participating will be Mr. Stephen Du Brul of the General Motors Corporation, Mr. Andrew Court of the National Recovery Administration, and Professor H. C. Carver of the University of Michigan.

The annual meeting of the Fellows of the Econometric Society will be held on December 27, at 12:30 P.M., in a private dining room of the Benjamin Franklin Hotel. Following luncheon there will be a business session.

In accordance with custom, the Econometric Society will also hold sessions with Section K of the American Association for the Advancement of Science. These will be held in Boston, on December 30, following the meetings at Philadelphia. The program will include a public address by Professor Wesley C. Mitchell, vice-president for Section K, and probably one or more joint sessions with engineering societies, the program being devoted to "Cost of Production." Headquarters of The Econometric Society will be at the Hotel Statler.

CHARLES F. ROOS
Chairman Program Committee

PAPERS TO APPEAR IN ECONOMETRICA

THE SURVEYS

J. TINBERGEN: Significant Developments in General Economic Theory (to appear January, 1934)

FELICE VINCI: Significant Developments in Business Cycle Theory (to appear April, 1934)

G. DARMOIS: Significant Developments in Statistical Technique (to appear July, 1934)

J. R. HICKS: Significant Developments in General Economic Theory (to appear January, 1935)

OTHER CONTRIBUTIONS TO APPEAR IN EARLY ISSUES

R. G. D. ALLEN: A Comparison Between Different Definitions of Complementary and Competitive Goods

LUIGI AMOROSO: Vilfredo Pareto

PROF. BONINSEGNI: Léon Walras

A. L. BOWLEY: Francis Ysidro Edgeworth

GRIFFITH C. EVANS: Maximum Production Studied in a Simplified Economic System

IRVING FISHER: The Income Concept

RAGNAR FRISCH: Robert Schmidt's Definition of Skewness and Kurtosis

RAGNAR FRISCH: Changing Harmonics Studied from the Point of View of Linear Operators and Erratic Shocks

L. FURLAN: Le volume maximum du crédit dans le cas d'un système économique isolé

PH. LE CORBEILLER: Oscillations autoentretenus dans le cas discontinu

GEORGES LUTFALLA: Fisher et Laurent, inventeurs des fonctions d'indice

L. HAMBURGER: Note on Relaxation Oscillations

JAKOB MARSCHAK: Econometric Parameters in a Stationary Society with Monetary Circulation

E. H. PHELPS BROWN: Demand Functions and Utility Functions. A Critical Examination of Their Meaning.

CHARLES F. ROOS: Theoretical Studies of Demand

ERICH SCHNEIDER: Statistical Construction of Cost Curves

ERICH SCHNEIDER: Heinrich von Thünen

EUGENE SLUTSKY: The Summation of Random Causes as a Source of Cyclic Processes

HANS STAEHLE: The Reaction of Consumers to Changes in Prices and Income. A Quantitative Study in Immigrants' Behaviour

F. A. VON HAYEK: Capital and Economic Fluctuations